The American Political Science Review

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Articles by Edward N. Muller, Andre Modigliani, Benjamin I. Page and Richard A. Brody, Charles S. Bullock, III, Robert L. Heilbroner

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TREASURY DEPARTMENT

Alexander L. George, I. M. Destler
Making Foreign Policy

Eugene F. Miller, David Braybrooke and Alexander Rosenberg, Richard S. Rudner, Martin Landau
Positivism, Historicism, and Political Inquiry

John Gerard Ruggie
Collective Goods and Future International Collaboration

Charles E. Butterworth
Averroës: Politics and Opinion

Donald G. Morrison and Hugh Michael Stevenson
Integration and Instability: Patterns of African Political Development

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ARTICLES

751 **The Case for Multiple Advocacy in Making Foreign Policy.** The system of multiple advocacy attempts to convert intraorganizational conflicts over policy into a balanced system of policy analysis and debate. This requires the executive to (1) structure and manage the policy-making system to ensure that there are advocates to cover the range of interesting policy options on a given issue; (2) equalize or compensate for disparities among the actors in the resources needed for effective advocacy; (3) identify and correct possible "malfunctions" in the policy-making process before they can have a harmful effect on the executive's choice of policy. Nine types of malfunctions are identified in this paper via critical diagnosis of U.S. foreign policy making in cases in which the executive had to decide questions of commitment, intervention, or escalation. Responsibility for identifying and correcting such malfunctions and for managing multiple advocacy effectively should be clearly fixed with the Special Assistant for National Security Affairs. However, the Special Assistant should not combine the role of "custodian-manager" of the policy-making system with the additional tasks of (a) policy adviser to the President; (b) public spokesman for existing policies; (c) "watch-dog" of the President's personal power stakes; or (d) implementer of policy decisions already taken. The attempt to do so invites serious role conflicts that can undermine the Special Assistant's performance of the all-important task of custodian.

By ALEXANDER L. GEORGE, Professor of Political Science, Stanford University.

Comment. By I. M. DESTLER, Research Associate in Foreign Policy Studies, The Brookings Institution.

Rejoinder. By ALEXANDER L. GEORGE.

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796 **Positivism, Historicism, and Political Inquiry.** The present controversy between "behavioral" and "postbehavioral" views of political inquiry reflects a larger dispute between two opposing theories of knowledge. Whereas the behavioral movement has its epistemological roots in positivism and, ultimately, in classical British empiricism, the most recent protest against behavioralism draws upon the theory of knowledge that has been the principal foe of empiricism over the past century. This theory of knowledge, which received the name "historicism" shortly after its emergence, had become the dominant epistemological position by the mid-twentieth century. This essay considers the general nature of historicism and its influence on the recent revolt against positivism in the philosophy of science. Finally, it examines the use that political scientists have made of historicist principles in opposing positivistic models of political inquiry. It argues that an epistemological relativism becomes unavoidable once certain premises of historicism are embraced.

By EUGENE F. MILLER, Associate Professor of Political Science, University of Georgia.

Comment. By DAVID BRAYBROOKE, Professor of Philosophy and Politics, and ALEXANDER ROSENBERG, Assistant Professor of Philosophy, Dalhousie University.

Comment. By RICHARD S. RUDNER, Professor of Philosophy, University of Washington, St. Louis.

Comment. By MARTIN LANDAU, Professor of Political Science, University of California, Berkeley.

Rejoinder. By EUGENE F. MILLER.

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874 **Collective Goods and Future International Collaboration.** Certain institutional changes in the advanced industrial societies are said to be leading toward postindustrial, if not postmodern, forms of sociopolitical organization. Scientific and technological developments are usually seen as the generating forces of such change, a major dimension of which is thought to be a growing preoccupation with goods and services which are produced and/or purchased communally.

There exists a parallel phenomenon in the modern interstate system, namely, a growing incidence of joint production and joint regulation by states, much of it in scientific and technological fields. Factors both leading to and limiting such joint activities, and some consequences of different kinds of collective decision making and administrative arrangements for the interstate system, are here explored.

It is an explicit aim of this inquiry, in addition, to avoid the evolutionary or functionalist assumptions informing much of the contemporary study of international organization. Instead, I argue that the processes of international organization are generated by how and why states choose from among alternate modes of performing tasks, both national and international, under varying conditions of possibilities and constraints. The bulk of the article develops and illustrates permutations of this basic posture.

The analysis suggests a number of future modifications of the modern interstate system, and of the modern state as an actor in that system. But these modifications share little with the kinds of international arrangements and structures past theories have led us to expect.

By John Gerard Ruggie, Acting Assistant Professor of Political Science, University of California, Berkeley and Research Fellow, Center for International Affairs, Harvard University.

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894 **Averroës: Politics and Opinion.** Averroës is primarily known for his numerous commentaries on Aristotle, yet his most explicitly political writing was presented in the form of a commentary on Plato's *Republic*. Consequently, the major dispute in scholarly literature has been whether he was more of a Platonist or an Aristotelian, a dispute of concern only because it reflects the important political question of what Averroës thought about the relation between theory and practice. This essay seeks to answer that question by studying the numerous editions and translations of his writings made available by contemporary scholarship. His commentaries on the logical arts concerned with public speech and common opinion are first examined, and the teaching set forth there is then contrasted with the kind of ideas Averroës expounded in his explicitly public works.

In the commentaries on the logical arts, Averroës stretched Aristotle's arguments in order to suggest that unexamined opinion was an insufficient guide for the practical arts dependent on such opinion. Although Averroës did not claim that such arts could be guided only by theoretical knowledge, he did insist that they could be best used only by those aware of the limits of opinion. This opening for the guidance of philosophy was carried further in the public writings where the need for philosophy to direct sound practice was defended against religiously motivated attacks on philosophy. Thus, the correct understanding of Averroës's views about the relation between theory and practice is closely related to an appreciation of his views about the role peculiar to philosophy and philosophers in his own religious community.

By Charles E. Butterworth, Assistant Professor of Government, University of Maryland.

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902 **Integration and Instability: Patterns of African Political Development.** This paper discusses the construction of a formal model of national political development derived from theories of political integration and instability, and reports the results of tests of the model based on data descriptive of contemporary black African nations. Political integration is conceptually elaborated in terms of processes of horizontal, vertical, and value integration, and political centralization. Political instability is conceptualized in terms of elite, communal, and mass instability. These dimensions of integration and instability are operationalized, and the analysis evaluates the hypothesis that integration decreases the likelihood of political instability in African nations, and that political centralization, in particular, decreases the likelihood of political instability by modifying, or reinforcing, the effects of other processes of integration. Methodologically, the analysis is based on the assessment of convergent validation for hypotheses tested with multiple indicators, regression, and path-analytic techniques.

By Donald G. Morrison, Visiting Director of the Computing Center, University of Ibadan and Hugh Michael Stevenson, Assistant Professor of Political Science, York University.

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928 **A Test of a Partial Theory of Potential for Political Violence.** Potential for political violence is defined by a summated scale built from two cumulative scales measuring approval of and readiness to engage in behaviors which constitute progressively greater challenge to a political regime. A prevalent explanation of potential for political violence proposes that the major direct antecedent is relative deprivation. The partial theory proposed here does not assign a central role to relative deprivation; rather, diffuse support for the political authority structure, and belief that political violence has led to goal attainment in the past, are proposed as major direct antecedents. Relative deprivation is defined by position on the Cantril Self-Anchoring scale with respect to three welfare values. The measure of diffuse affect for the political authority structure is a weighted summation of items measuring the degree to which political authorities are believed to wield power honestly, benevolently, and justly. Belief that political violence has led to goal attainment is defined as a summation of items measuring the degree to which the use of political violence by dissident groups is thought to have been helpful. The data base is a sample of a population in which instances of political violence have been frequent in the past. A linear additive model of Potential for Political Violence, with Trust in Political Authorities and Efficacy of Past Violence as describing variables, shows an accuracy of prediction which is satisfactory and superior to that yielded by a multiplicative model.

By Edward N. Muller, Assistant Professor of Political Science, State University of New York at Stony Brook.

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960 **Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy.** Recent studies of public opinion on Vietnam reveal a surprising positive association between socioeconomic status (SES) and support for "tougher" military policies. In an effort to extend and understand such findings, this study reanalyzes several public opinion surveys conducted during the Korean War. The demographic correlates of military policy preferences are found to

be similar to those reported for the Vietnamese War, but the Korean data qualify the proposition that higher SES is associated with support for "tougher" policies in two respects: (1) policy preferences of the Korean public did not appear to be organized on a simple "soft-to-tough" dimension—those who supported escalation were only slightly more likely than average to oppose disengagement; (2) while SES was positively associated with opposing disengagement, it was not associated with supporting escalation. Factor analyses of several surveys reveal two distinct, orthogonal dimensions that underlay military policy preferences: an "isolationist-to-interventionist" dimension associated with opposition to disengagement; and a "trust-to-distrust of the Truman Administration" dimension associated with support for escalation. Respondents are subdivided into four types in accordance with their positions on these dimensions. The demographic characteristics and military preferences of the four types are analyzed, and the overall findings are discussed with reference to Vietnam as well as public opinion on military policy more generally.

By ANDRE MODIGLIANI, Associate Professor of Sociology, University of Michigan.

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979 **Policy Voting and the Electoral Process: The Vietnam War Issue.** The infrequency of issue voting in American presidential elections is usually attributed to a lack of policy rationality among voters. An examination of the Vietnam war issue in 1968 suggests, however, that much of the explanation may lie instead with the electoral process itself, and with the kinds of choices which are offered to citizens.

Policy preferences concerning Vietnam were only weakly related to the two-party vote. Less than 2 per cent of the variance in voting choices between Nixon and Humphrey could be accounted for by opinions on Vietnam. Yet the absence of issue voting could not be fully explained by voters' failings. Most people had strong opinions about Vietnam. The public was generally able to perceive where prenomination candidates stood on the issue. People were able and willing to take account of Vietnam in evaluating other candidates.

Voters did not bring their Vietnam preferences to bear upon the choice between Nixon and Humphrey because they saw little difference between the positions of the two, and because they were not certain precisely where either one stood. These perceptions, in turn, were rooted in reality. Humphrey's and Nixon's campaign speeches show that they did differ rather little on specific proposals about Vietnam. Further, both candidates indulged in so much ambiguity about Vietnam that public confusion over their positions was understandable.

There are theoretical reasons for believing that candidates in a two-party system often have an incentive to converge at similar policy positions, and to be vague. If they generally do so, their behavior may contribute significantly to the apparent nonrationality of voters. In addition, it may have important implications for questions of collective rationality and social choice.

By BENJAMIN I. PAGE, Assistant Professor of Political Science, Dartmouth College and RICHARD A. BRODY, Professor of Political Science, Stanford University.

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996 **Freshman Committee Assignments and Re-election in the United States House of Representatives.** Two students of the committee assignment process, Nicholas Masters and Charles Clapp, as well as some congressmen, assert that the most crucial factor in filling committee vacancies is whether the appointment will enhance the recipient's chance of re-election. This statement is tested using data for Republican and Northern Democratic freshmen elected to the House between 1947 and 1967.

The freshmen are grouped on the basis of assumptions about which assignment or assignments should help them win re-election. When narrowly elected and safe freshmen are compared, there is no evidence that the former more frequently receive assignments likely to facilitate re-election. Thus there is no support for the Masters-Clapp proposition.

Investigation further reveals that even those freshmen from marginal districts who are awarded "good" appointments are not re-elected significantly more often than are comparable newcomers having less favorable assignments. Committee assignments therefore seem relatively unimportant in determining whether a congressman wins a sophomore term. Indeed, more than 70 per cent of the freshmen who triumphed in hotly contested races to reach the House are returned. When these incumbents are defeated it is typically as a result of nation-wide forces over which they exercise little if any control.

The implications of this research are that congressmen have a greater range of alternatives than is often thought. Even the narrowly elected novice is relatively free to seek appointment to committees for reasons other than constituency service or promotion. Largely symbolic activities are available through which concern for the district and its problems and needs can be demonstrated, thereby freeing much of the congressman's time and attention to pursue other less parochial goals.

By CHARLES S. BULLOCK, III, Associate Professor of Political Science, University of Georgia.

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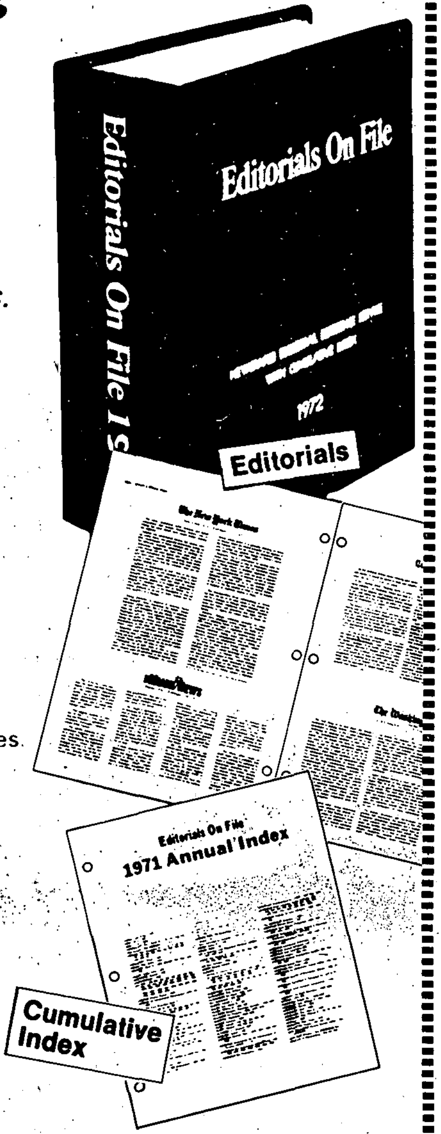

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The Case for Multiple Advocacy in Making Foreign Policy*

ALEXANDER L. GEORGE
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This paper discusses prescriptive theories of policy making in complex organizations with particular reference to foreign policy making in the Executive Branch. Instead of utilizing centralized management practices to discourage or neutralize internal disagreements over policy, an executive can use a multiple advocacy model to harness diversity of views and interests in the interest of rational policy making. Diversity is also given scope in "bureaucratic politics" and "partisan mutual adjustment," but in contrast to these unregulated pluralistic systems, multiple advocacy requires management to create the basis for structured, balanced debate among policy advocates drawn from different parts of the organization (or, as necessary, from outside the organization). Multiple advocacy, therefore, is a "mixed system" that combines elements of a centralized management model with certain features of pluralistic and participatory systems.

The functional equivalent of a rational model can be provided within an organization also by means other than multiple advocacy. One such organizational device, that of "adversary proceedings," is often recommended as a means for obtaining a final critical examination of a preferred policy option before it is recommended to or adopted by the top decision maker.[1] The concept of "adversary proceedings," borrowed from the judicial system, is not a well-defined or standardized method for organizational decision making. Generally, however, it includes few advocates, and covers fewer options than would a system of multiple advocacy.[2]

Another organizational device for obtaining a rational consideration of alternatives is the "devil's advocate." Strictly speaking, the devil's advocate performs a role; it is understood that he will argue on behalf of an unpopular option which should be considered but which no one else will speak up for, and which the devil's advocate himself does not really favor. A strong case can be made in principle for utilization of a devil's advocate;[3] but one cannot be sanguine about its efficacy in foreign policy making[4] on the basis of recent experience.

Still another, and far more comprehensive, organizational approach for achieving the functional equivalent of the ideal rational model can be seen in President Nixon's version of the National Security Council. Unlike a multiple advocacy system, which defines the role of the chief executive as a "magistrate" who listens in a structured setting to different, well-prepared advocates making the best case for alternative options, the present NSC relies upon staff work to distill, analyze, and present options to the President for his choice. Thus, the President is defined as a unitary rational decision maker who is shielded from too much direct exposure to undigested, "raw" disagreements over policy within the executive branch.

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* So many persons have commented on earlier drafts of this paper that it is difficult to list all of them in acknowledging my debt and appreciation. I want to thank the panel chairman, Paul Hammond, and Allen Schick and William Harris for their helpful discussion of an earlier version of this paper that was given at the annual meetings of the American Political Science Association in Chicago, September 1971. I would like to express appreciation also to the Committee on International Studies, Stanford University, for its support of my research.

[1] See, for example, the recommendation that an adversary system be adopted at various echelons within the State Department, in Task Force VII, "Stimulation of Creativity," *Diplomacy for the 70's: A Program of Management Reform for the Department of State.* Department of State Publication 8551, series 143. December 1970. U.S. Government Printing Office, Washington, D.C.

A laboratory exploration of the possible advantages for administrators of an adversary proceeding between technical experts is reported by William Fairley and Frederick Mosteller, "Trial of an Adversary Hearing: Public Policy in Weather Modification" (draft), Harvard University, May 10, 1971.

[2] For a discussion and evaluation of the judicial model from the standpoint of decision-making theory, see Richard O. Mason, *Dialectics in Decision-Making: A Study in the Use of Counterplanning and Structured Debate in Management Information Systems,* Ph.D. dissertation, Business Administration, University of California, Berkeley, 1968, pp. 81–4.

[3] See, for example, Joseph H. de Rivera, *The Psychological Dimension of Foreign Policy* (Columbus, Ohio: Charles E. Merrill, 1968), pp. 61–4, 209–11; also, Robert Jervis, "Hypotheses on Misperception," *World Politics,* 20 (April, 1968), 464–5; R. F. Kennedy, *Thirteen Days* (New York: Norton, 1969), p. 112; Ole R. Holsti, *Crisis, Escalation, War* (Montreal and London: McGill-Queen's University Press, 1972).

[4] James C. Thomson, Jr. provides an insightful and sobering account of the "domestication" of devil's advocates in Vietnam policy making. ("How Could Vietnam Happen? An Autopsy," *Atlantic Monthly* [April, 1968] pp. 47–53.) The efficacy of a devil's advocate is firmly discounted also by George Reedy, the one-time press secretary to President Johnson, in *The Twilight of the Presidency* (N.Y.: World, 1970), p. 11.

I have dealt more fully with the uses and limitations of devil's advocates in U.S. foreign policy making in "Stress Adaptation in Political Decision-making: The Individual, Small Group, and Organizational Contexts," in *Coping and Adaptation,* ed. George V. Coelho, David A. Hamburg, and John Adams (forthcoming).

Both the Nixon NSC concept and multiple advocacy attempt to cope with those stubborn dynamics of organizational behavior and bureaucratic politics that undermine or destroy efforts to achieve the functional equivalent of rationality in making foreign policy decisions; but they do so in different ways. Each model has somewhat special requirements and conditions for reasonably effective performance. Before contrasting these two approaches in further detail, one should note that the burden of evaluating prescriptive policy-making models is a heavy one. Only very limited empirical evidence exists for either model.[5] The task of comparative evaluation is complicated in any case by several factors: The reality of a policy-making system is always more opaque and inconsistent than the theoretical model which rationalizes it. Moreover, while it is useful at this stage in theory development to sharpen the differences between alternative prescriptive models, certain features of the multiple advocacy system to be discussed in this paper do appear in some form and under certain circumstances in the present NSC system; and still other prescriptions for managing foreign policy making, to be discussed later under "preventive interventions," could be incorporated into the Nixon-Kissinger model without robbing it of its distinctive features.

It is important, finally, to recognize that neither multiple advocacy nor any other management model can be expected to cope reliably with many constraints on the quality and effectiveness of foreign policy. In focusing in this paper on the way in which the policy-making system is organized and operated—i.e., the process variables—we do not ignore or underestimate the simultaneous importance of many other decision-making variables: ideology, cognitive beliefs about opponents and about the nature of the international system, operational codes, public opinion, substantive knowledge and skill (relevant, for example, to negotiation, deterrence, coercive diplomacy, crisis management, conciliation, alliance-maintenance, etc.). Because many other variables of this kind also influence the quality of decision making, even a good policy-making process—i.e., one that provides the functional equivalent of a rational model of decision making—cannot guarantee that highly adaptive decisions will be made in every case. Rather, it is a matter of probabilities, and particular attention should be given to the ability of a policy-making system to do well when dealing with critical rather than routine decisions.

One set of critical decisions in foreign policy concerns decisions involving commitment, intervention and escalation—the class of foreign policy decisions singled out for analysis in this paper. Ideological premises and cognitive beliefs about the opponent and about the nature of the international system held by policy makers are likely to play a particularly important role in determining such decisions. Indeed such values and beliefs may "dominate" on occasion whatever independent contribution even a well-designed and well-managed policy-making system can make to the quality of decisional outputs.

It is not easy to deal with these essentially normative problems in focusing on different ways of organizing the process variables of policy making, although we shall attempt to do so to some limited extent when we review historical cases in U.S. foreign policy. For the moment it may be appropriate simply to remind ourselves that these other variables also affect foreign policy decisions made via highly centralized policy-making systems such as the present NSC. To the extent that there is significant *disagreement* among policy makers on matters of foreign policy ideology and related cognitive beliefs, one can argue that a more openly competitive system such as multiple advocacy is more likely to secure a critical examination and weighing of them, outside of as well as inside the executive branch, than a highly centralized policy-making system. But we confess that there is little empirical evidence to clarify, let alone settle this question. Indeed, the performance of policy-making systems has hardly been studied from this broader standpoint. Rather, the framework usually employed regards values and beliefs of this kind as decisional premises that bound the more narrow search for "efficient" means.

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[5] Even less can be said about the merits and drawbacks of different foreign policy-making models than about alternative prescriptive models of industrial management, which continue to be the subject of controversy among social psychologists despite considerable research efforts to clarify their effectiveness. For an incisive analytical summary of this controversy, useful in several respects for the present discussion, see Victor H. Vroom, "Industrial Social Psychology," in *Handbook of Social Psychology*, ed. Gardner Lindzey and Elliot Aronson, 2nd edition, Vol. 5 (Reading, Mass: Addison-Wesley Publishing Company, 1970) esp. 227–240.

The National Security Council: Some Questions

We shall forego a detailed description of President Nixon's reorganization of the National Security Council in 1969.[6] It will suffice for our purposes to articulate some of the underlying premises of the current NSC model and to raise some questions of theoretical and practical interest. It should be emphasized at the outset, however, that the administration does not always make foreign policy decisions in a manner consistent with its own NSC model. Any president and any administration is likely to resort to a number of different ways of making major foreign policy decisions. Even multiple advocacy can be noted from time to time in the present administration's way of making certain foreign policy decisions.[7] And, as a sympathetic observer recently noted, "The NSC system today is not the tidy blueprint of January, 1969. The older it has gotten, the more informal and overlapping its procedures have become."[8]

One of the major objectives of Nixon's reorganization of the NSC was to curb, if not altogether eliminate, the play of "bureaucratic politics" in the making of foreign policy within the executive branch. By instituting centralized control and direction of the search for and evaluation of options at the NSC level, Nixon has greatly enhanced presidential control over foreign policy decision making and weakened the ability of officials in departments and agencies to exercise independent judgment and influence over decisions. President Nixon and his Special Assistant for National Security Affairs, Henry Kissinger, have tried to impose the model of unitary, rational policy maker on the more loosely structured pluralistic systems of previous administrations.

It must be recognized that the present NSC system has indeed reduced some of the dysfunctional consequences associated with the freer play of "bureaucratic politics" in more loosely coordinated policy-making systems of earlier periods. As in other types of complex organizations, subunits of the executive branch tend to engage in "quasi resolution of conflict" and to avoid uncertainty in relations with each other by means of "negotiating the internal environment" within the organization.[9] Left to their own devices, those subunits which share responsibility for a particular policy area often adapt by restricting competition with each other. As a result, policy issues may not rise to the presidential level, or when they do, they often take the form of concealed compromises that reflect the special interest of actors at lower levels of the hierarchical system.

Another dysfunctional aspect of relatively decentralized "bargaining" systems of policy making stems from the tendency of an individual subunit to restrict its contribution to the organizationwide search for and evaluation of options in order to enhance its ability to influence top-level organizational choice. Thus, a subunit (for example, the State Department or the Joint Chiefs of Staff) might attempt to provide the chief executive with only one preferred option, which reflects its own internal bargaining politics and obscures or conceals information and analysis that support other options.[10]

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[6] For detailed description of President Nixon's reorganization of the NSC see, for example, Vincent Davis, "American Military Policy: Decision-making in the Executive Branch," *Naval War College Review* (May 1970), pp. 4–23; Robert H. Johnson, "The National Security Council: The Relevance of Its Past To Its Future," *Orbis*, 13 (Fall, 1969), 709–735; E. A. Kolodziej, "The National Security Council: Innovations and Implications," *Public Administration Review*, 29 (Nov.-Dec. 1969), 573–585; Frederick C. Thayer, "Presidential Policy Processes and 'New Administration': A Search for Revised Paradigms," *Public Administration Review*, 31 (September/October 1971) 552–561; and particularly Irving M. Destler, *Presidents, Bureaucrats and Foreign Policy* (Princeton, N.J.: Princeton University Press, 1972).

For accounts of the historical evolution of the NSC see, for example, Keith C. Clark and Lawrence J. Legere, eds., *The President and the Management of National Security* (N.Y.: Praeger, 1969); Paul Y. Hammond, *Organizing for Defense* (Princeton: Princeton University Press, 1961); Senator Henry M. Jackson, ed., "Jackson Subcommittee Papers on Policy-Making at the Presidential Level," *The National Security Council* (N.Y.: Praeger, 1965); Stanley Falk, "National Security Council under Truman, Eisenhower, and Kennedy," *Political Science Quarterly*, 79 (Sept., 1964), 403–434.

[7] As, for example, in the policy planning for the SALT talks. See Samuel C. Orr, "Defense Report/National Security Council Network Gives White House Tight Rein over SALT Strategy," *National Journal*, 3 (April 24, 1971), 877–886; and John P. Leacocos, "Kissinger's Apparat," *Foreign Policy*, No. 5 (Winter 1971–72), p. 19.

[8] Leacocos, p. 5.

[9] Richard M. Cyert and James G. March, *Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963), pp. 116–120. As Robert Axelrod (*Conflict of Interest* [Chicago: Markham, 1970, p. 123]) points out, Cyert's and March's observation regarding internal bargaining and other adaptive devices on the part of the subunits of an organization "leaves open the question of how the upper levels of the organization supervise the sub-units. . . ." This is not surprising, however, since the Cyert-March theory is not a prescriptive management theory.

[10] This criticism of the State Department was tacitly accepted by its Task Force VII, "Stimulation of Creativity," in noting that "the lack of a system for subjecting policy to the challenge of an adversary view" within the Department was "a major weakness

These and other dysfunctional aspects of "bureaucratic politics" were well known to President Nixon when he took office; his desire to avoid them influenced the redesign of the NSC. As he put it in his "U.S. Foreign Policy for the 1970's":

> The new NSC system is designed to make certain that clear policy choices reach the top, so that various positions can be fully debated in the meeting of the Council. Differences of view are identified and defended, rather than muted or buried. I refuse to be confronted with a bureaucratic consensus that leaves me no options but acceptance or rejection, and that gives me no way of knowing what alternatives exist.[11]

Indeed, under Kissinger, presidential-level involvement in, and efforts to control foreign policy formation and implementation have substantially increased. The staff of the NSC, augmented in numbers and functions, has reached deeper into the departments and bureaus in order to identify and gain control over a wider range of issues at earlier stages. What is more, strengthening a trend begun in the Kennedy administration, the Special Assistant for National Security Affairs has emerged as a semi-autonomous actor in the policy system. Kissinger now plays an active, central role in influencing and shaping the process of policy analysis throughout the executive branch in order to formulate well-developed options for the President's consideration. The enhanced role of the Special Assistant incorporates important features of the management model developed earlier by Secretary of Defense McNamara in the sphere of defense planning.[12] Thus, reminiscent of McNamara is Kissinger's practice of frequently initiating policy studies from the presidential level (the well-known National Security Study Memoranda, or NSSM), as a means of enabling the NSC staff to force issues that would otherwise be buried or neglected by the agencies up for ultimate presidential action.[13]

If we grant that the present system curbs some dysfunctional features of "bureaucratic politics," we are still left with the question of the cost and limitations of so centralized a system. To be sure, interested personnel in the departments, agencies, and subunits are by no means excluded from participating in the present policy-making process. In fact, in some respects, via their membership in the various NSC-centered committees, many departmental personnel are involved more systematically and more closely at least in preparatory stages of policy making than was the case under previous administrations.[14] At the same time, however, the roles of leading departmental officials have been subtly redefined and delimited; indeed, cabinet officials and other senior officials *have been seriously weakened in their roles as policy advocates and advisers*. While this may not have been intended by Nixon and Kissinger, it is nonetheless a consequence of the way in which they have restructured the system. In the course of developing and evaluating policy options, the intricate machinery of the NSC coopts foreign policy specialists and analytical resources in the departments. As a result, when department heads and senior officials finally do have an opportunity at top-level NSC meetings with the President to express their views on alternative options, as indeed the present system permits and encourages, they may not be in a position to offer well-considered *departmental* points of view backed by solid independent analysis.[15] In this weakening of independent analytically-oriented staffs at department levels, even senior departmental officials are placed at a disadvantage in performing as advisers to the President in the final stages of decision making. The question arises, therefore,

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in the Department's organization. . . ." (*Diplomacy for the 70's*, p. 294).

[11] Richard Nixon, *U.S. Foreign Policy for the 1970's: A New Strategy for Peace*, A report to the Congress, February 18, 1970 (Washington, D.C.), p. 22. Henry Kissinger had earlier noted and deplored the play of bureaucratic politics in policy making in "Domestic Structure and Foreign Policy," *DAEDALUS*, 95 (Spring, 1966), 503–529.

[12] Alain C. Enthoven and K. Wayne Smith take note of the fact that the NSC in the Nixon Administration has been made "the focal point for reviewing alternatives and formulating broad national security policy." They add that the NSC staff under Kissinger "appears to be trying to do for the President at the national level what the Systems Analysis office did for the Secretary of Defense." (*How Much Is Enough? Shaping the Defense Program, 1961–1969* [N.Y.: Harper & Row, 1971], p. 332).

[13] Destler credits the NSSM device with a particularly important contribution to Nixon's decision to renounce use of biological weapons. "Only the Army really wanted them, and, within the Army, mainly the branch specializing in them. But . . . the issue never got to the President for his review because neither the Secretary of Defense nor the Secretary of State was willing to pay the bureaucratic political price of recommending a change" (Destler, *Presidents, Bureaucrats, and Foreign Policy: The Politics of Organizational Reform*, Ch. 5).

[14] This is evidently the case, for example, with regard to the Arms Control and Disarmament Agency. See Orr, "Defense Report" and Leacocos, "Kissinger's Apparat," p. 19.

[15] This is implied in several accounts of the Cambodian decision. See Hedrick Smith, "Cambodian Decision: Why President Acted," *N.Y. Times*, June 30, 1970; David Maxey, "How Nixon Decided to Invade Cambodia," *Look*, August 11, 1970.

whether the present system suffers from overcentralization and overbureaucratization of the preparatory "search" and "evaluation" phases of policy analysis.

It should be noted also that the present system attempts the questionable procedure of separating the "search" and "evaluation" phases of policy making as much as possible from the final process of "choice."[16] While providing orderly procedures for widespread participation of foreign policy specialists in the departments and agencies in the "search" and "evaluation" phases, the present NSC system restricts the *process* of "choice," and not merely the final decision, essentially to the President. Those few advisers who participate in the final discussions before he makes his choice of policy all largely depend on the same body of distilled analysis of options that has emerged from the centralized, "advocate-free" search and evaluation process. The question can be posed whether the executive would be in a position to make better decisions if *multiple centers of analysis* and *stronger staffs* were available to senior departmental officials who serve as advisers to the President in the final stages of foreign policy decisions.[17]

A number of other questions can be raised about the present NSC system. While it is nicely suited to Nixon's preference for solo decisions made in private,[18] such a system tends to exaggerate the all-too-familiar risk that arises whenever a president improvises at the last minute an option somewhat different from the carefully evaluated ones presented to him. As is well known, the utility of a complex option often suffers from last-minute alterations of some of its components. Unless the President's improvised choice is resubmitted for careful evaluation, the value of the NSC's meticulous preparation of options can be easily defeated. This danger is enhanced in crises when time pressures for decision are urgent.

More generally, when decisions must be arrived at quickly the NSC's orderly but somewhat laborious collection and evaluation of "pure," advocate-free options from subpresidential levels in the government may break down or work ineffectively. Evidently for this reason the "Washington Special Actions Group" (WSAG), chaired by Kissinger, was established within the structure of NSC committees to deal with crises. Relatively little information is available on WSAG's performance, the unauthorized disclosures to columnist Jack Anderson of its handling of the Indian-Pakistan conflict in late 1971 being an important exception. Kissinger himself has conceded that while the NSC system "works rather well" on the whole, it does "better in noncrisis situations than in crises."[19] It would seem particularly important for crisis management, therefore, to back-stop the NSC apparatus with a system of multiple advocacy in which representatives of departments and agencies can draw upon competent analytical staffs of their own.

A highly centralized policy-making system such as the present NSC system can easily overburden the top-level decision maker. It may be necessary, as has often been said, to centralize first before effective decentralization is possible. With the passage of time, however, signs have multiplied that the present NSC suffers from dangerous overload precisely because of its inability to build "centers of strength responsive to the President in other parts of the foreign affairs government."[20]

The NSC system gives the President and his Special Assistant, Henry Kissinger, opportunities to use their control over the NSC committees to shape final policy choices, if they wish to, during the course of the centrally directed and coordinated policy analysis. One observer concludes a balanced appraisal of the workings of the NSC system with the observation that, "The kinds of analyses that are done, the way the choices are presented to the President and the NSC, and the shape of the resulting policies inevitably reflect the biases of the President and his leading officials, such as Kissinger."[21] Another observer, in an otherwise sympathetic evaluation of the NSC, asks whether the often-repeated phrases such as "keeping the options open" do not have "more a liturgical than intellectual significance"; he also notes that some critics complain that the NSC-initiated studies impose busywork on the departments while the Special Assistant and his staff focus on the essential issues.[22]

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[16] For a general discussion of the desirability of keeping specialists carrying out search, analysis, and choice close together within an organization, and also of the conditions under which it is "rational" to separate them, see Anthony Downs, *Inside Bureaucracy,* (Boston: Little, Brown & Co., 1967), pp. 179-188. (Downs uses the term "evaluation" for what is here referred to as "choice.")

[17] This point is also emphasized from a somewhat different standpoint by Destler in *Presidents, Bureaucrats, and Foreign Policy.*

[18] Destler, Ch. 5.

[19] Quoted by Milton Viorst, "William Rogers Thinks Like Richard Nixon," *New York Times,* Magazine, February 27, 1972. On the NSC's Washington Special Actions Group, see also Leacocos, "Kissinger's Apparat," pp. 7–8.

[20] Destler believes the failure to do so is one of the major deficiencies of the Nixon Administration's organization of foreign policy making. The creation of the Planning and Coordination Staff in the State Department in 1969 has evidently not been as successful in this respect as had been hoped. In mid-1971 a new system of evaluating country programs (Policy Analysis Resource Allocation) was installed within State having as its goal to give State more weight in the implementation of foreign policy (Leacocos, "Kissinger's Apparat," p. 10).

[21] Orr, "Defense Reports," p. 881.

Moreso than in any previous administration the Special Assistant for National Security Affairs is now increasingly one of the President's leading foreign policy advisers if not the most important one. Thus the basis has been created for a serious role conflict that can interfere with the performance of his other task as "custodian-manager" of the process of policy formulation.

It should be noted, finally, that by placing the President at the helm of a more tightly controlled, depoliticized system of policy making within the executive branch, the present NSC system has also strengthened his position in the foreign policy arena vis-à-vis Congress. For by weakening the struggle over foreign policy within the executive branch, the new NSC system also limits the opportunities "outside" actors (Congress) have had to influence the making of policy by participating informally in the play of "bureaucratic politics" among the various actors in the executive branch.

While many questions can be raised about the performance of the present NSC system and the prescriptive model of rational organizational decision making that guides it, it is by no means easy to formulate an overall appraisal. In any case it is unlikely that a future president will decide whether to continue with this type of NSC system exclusively on the basis of available evaluations of its performance under the present administration. Rather, the variable that will probably dominate in shaping the NSC in a new administration, as a long-time staff member and student of the NSC reminds us, will be "the President's style of decision-making."[23] Variation in the personal styles of past presidents helps account for the zigzags in degree of institutionalization and reliance upon NSC procedures. These personal preferences are themselves likely to lead to reorganization of the NSC in the future, even apart from the deeply ingrained American faith in the virtues of organizational tinkering.

Assuming a disposition to return to a somewhat more loosely coordinated, pluralistic system of internal policy making at some point in the future, we must ask whether appropriate design models and management theories exist for that purpose. I have not been able to find any reasonably detailed ones in the literature on organization theory. It is well to make clear that we are talking about *prescriptive* theories that indicate how policy-making processes in complex organizations can be designed and managed to realize the presumed benefits of internal diversity and competition. Most theories of organizational behavior are of an explanatory-predictive type and do not provide much by way of guidelines or rigorous support for prescription. Conversely, normative theories of organizational decision making lack a firm empirical base.[24]

Converting "Bureaucratic Politics" Into Effective Multiple Advocacy

We have already noted some of the dynamics of organizational behavior and bureaucratic politics that undermine efforts to achieve rational consideration of policy and decision making. As a result of these barriers, many executives and scholars take a dim view of bureaucratic politics; they deplore the fact that important policy decisions are influenced by negotiation and bargaining within the policy-making circle rather than being settled through more detached intellectual analysis. Other students of government, including the present writer, see possible merit in competitive policy making. They believe that conflict is normal, perhaps inevitable, and potentially healthy; for them, internal disagreement about policy within the organization is not necessarily an abnormal strain that must be abolished in the interest of rational decision making. Rather, conflict may help produce better policy *if* it can be managed and resolved properly.

Indeed, it is generally agreed that social conflict can promote adaptation. Social theorists of conflict have not extended their analysis of this phenomenon to decision-making systems,[25] but laboratory studies of decision-making groups provide some evidence that conflict within the group may have a constructive effect on its problem-solving activity and on the quality of its choices.[26] Perhaps the most interesting of these laboratory studies for our purposes is that by Joseph L. Bower, which is distinguished by the fact that it employs a multiactivity model of decision-making functions—the distinction between "search," "analysis," and "choice"[27]—that has been the focus of some of the most useful contemporary theories of organizational behavior. This distinction has already been employed in this paper to identify some of the essential features of the Nixon NSC concept and to differentiate it from the multiple advocacy model.

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[22] Leacocos, p. 23.

[23] Robert H. Johnson, "National Security Council," p. 720.

[24] See the discussion by Joseph L. Bower, who argues for a synthesis of normative and descriptive theory on the grounds that "both normative and descriptive content are prerequisites for prescription" ("Descriptive Decision Theory from the 'Administrative' Viewpoint," in Raymond A. Bauer and Kenneth J. Gergen, eds., *The Study of Policy Formation* [N.Y.: Free Press, 1968]).

[25] There is no reference to decision making, for example, in Lewis A. Coser's *The Functions of Social Conflict* (Glencoe: Free Press, 1956). Similarly, there is little direct interest in the quality of organizational decision making in William A. Gamson's otherwise stimulating analysis of the ways in which authorities can control discontented partisans seeking change (*Power and Discontent*, Homewood, Ill.: Dorsey Press, 1968).

Such a multifunction model of the decision-making process within a group or an organization seems essential if the effect of conflict on the quality of the decisional outputs is to be systematically evaluated *and* explained. Thus, in his study of experimental groups, Joseph Bower found that conflict (which he defined as differences in the utility functions of individual members of the group) was an important factor motivating constructive thought and analysis. The effect of conflict on "search" and "analysis" was particularly noteworthy. Under certain conditions the quality of search and analysis may be significantly improved by the presence of disagreement within the group. "The personal commitment of a subject to an initial position," Bower notes, "motivates him to defend his choice by presenting all the information which supports his position . . . group search is stimulated in both extent and quality." The value of having individuals with different utility functions in a decision-making group is highlighted by Bower's additional finding that groups were not successful problem solvers when they were solely concerned with "what is best for the group." This finding, as Bower notes, contradicts a major premise of other normative theories of social choice.[28]

These findings, and the multiactivity model of search, analysis, and choice on which they are based, are relevant for designing management models to cope with or capitalize upon the phenomenon of bureaucratic politics. Direct observation of the impact of bureaucratic politics on the quality of policy making indicates that sometimes it works poorly, producing decisions that are worse than they need be, while on other occasions it works surprisingly well, helping to formulate better policies as a result of the sharp give-and-take among multiple advocates. Accordingly, there is a need for case studies of the development of individual policies and decisions that will address this central question systematically and comparatively: under what conditions do the workings of bureaucratic politics help produce "good" decisions? And under what conditions do they tend to produce "poor" policies and decisions? What, in other words, are the prerequisites for effective functioning of the bureaucratic politics? Since such questions have not yet been studied systematically, only provisional answers are possible at this time.

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[26] A number of such studies have been conducted by N. R. F. Maier and his associates over a period of many years; they are reproduced and collated in Maier's *Problem-solving and Creativity in Individuals and Groups* (Belmont, Calif.: Brooks/Coles, 1970). See also R. L. Hoffman, "Conditions for Creative Problem Solving," *Journal of Psychology,* 52 (Oct., 1961) 429–444; Victor H. Vroom, Lester D. Grant, and Timothy S. Cotton, "The Consequences of Social Interaction in Group Problem Solving," *Organizational Behavior and Human Performance,* 4 (February, 1969), 77–95; Vroom, "Industrial Social Psychology," and Mason, *Dialectics in Decision-Making.*

While the simplified structure of these laboratory experiments precludes their findings from being directly transferable to governmental decision making, relevant structural and leadership variables have been studied, and some of the emergent propositions are consistent with impressionistic observations of critical variables that affect the performance of political decision-making groups in real life.

[27] As usually employed by organizational theorists such as Herbert Simon, James March, Richard Cyert, and others, *"search"* refers to the processes of obtaining and sharing relevant information, and of identifying and inventing alternative options; *"analysis"* (or "evaluation") refers to the processes of examining relationships among available information and evaluating the relative appropriateness of alternative options with reference to stated objectives and values; and *"choice"* refers to the procedures and decision rules followed in choosing from among the alternative options.

[28] Joseph L. Bower, "The Role of Conflict in Economic Decision-Making Groups: Some Empirical Results," *Quarterly Journal of Economics,* 79 (May, 1965), 263–277. For this study Bower simulated 58 capital budgeting committees, each of which was staffed by three Harvard MBA candidates. The groups were divided into "rational teams" and "foundations," the main difference between them being that all individuals in the first type of group had identical utility functions whereas members of the second type of group did not. One of the limitations of Bower's research study and of his findings stems from the fact that he employed and varied only two decision rules—unanimity and majority rule. Thus, Bower did not study the effect of a decision rule that is especially pertinent for government decision making, namely when the leader of the group (chief executive) makes the decision after group discussion, debate, bargaining, etc. Bower recognized that *severe* conflict within a group can have a debilitating effect on the probability of its reaching an agreement, but both of his two types of groups were evidently leaderless groups. The possibility suggests itself that groups with informal or authoritative leaders, operating with the third decision rule mentioned here, might succeed in reducing the debilitating effect of intra-group conflict on the probability of reaching agreement without, it should be added, also reducing the value of intra-group conflict for its performance of the problem-solving task.

Nonetheless, sufficient knowledge of the workings of bureaucratic politics has accumulated[29] to warrant efforts to formulate a variety of prescriptive management theories.[30] My own preference is to avoid narrowly prescriptive theories that focus on strategy and tactics ("How to be a more clever bureaucratic politics player") and to attempt, instead, the more ambitious and difficult task of developing normative-prescriptive theories of organizational policy making. Multiple advocacy is one such theory. It attempts to indicate how the policy-making system might be structured and managed so that internal disagreements might contribute to improving the quality of search and evaluation activities associated with choice of policy.

It should be made clear that as conceived here, multiple advocacy would be an integral part of a *mixed system* in which centralized coordination and executive initiative would be required. Multiple advocacy is *not* a plea for dispersing power more widely to subunits and actors within the bureaucratic politics model.

To reintroduce the prescriptive approach into the study of bureaucratic politics, in which Richard Neustadt pioneered with *Presidential Power,* we must return to leads provided by earlier scholars that have not been pursued more systematically in the past ten years. Curiously, the foremost advocate of the bargaining model of policy making, Charles Lindblom, while recognizing that it can and does perform badly on occasion, has displayed little interest in articulating the conditions necessary for its effective performance. Critics of the extreme bargaining model have been more helpful in this respect, but they have been content to argue the case for centralized management tools and stop well short of developing a theory for a sounder bargaining model.[31]

What, then, are the conditions and requirements for converting bureaucratic politics into effective multiple advocacy? In 1959 Roger Hilsman suggested some of the "conditions" under which the bureaucratic politics process might be rather efficient in identifying and assessing major policy alternatives in the realm of foreign policy. "The chances should be good," he suggested, "that the resulting policy will be a wise one. . . . If the subject under debate is one on which the different groups of advocates are knowledgeable, if the participants are well informed, and if the top levels of government participate fully in the process or are sensitive to the verdict. . . ."[32] Hilsman's observation was offered parenthetically and linked casually with a few illustrative cases. I will attempt a fuller statement:

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[29] Graham T. Allison has brilliantly codified and explicated much of the previous literature on bureaucratic politics by writers such as Lindblom, Neustadt, Schilling, Hammond, Huntington, Hilsman. The fullest and most recent report of Allison's work is his *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown & Co., 1971). (An earlier summary of his Ph.D. dissertation was published in *APSR,* September, 1969, under the title, "Conceptual Models and the Cuban Missile Crisis.")

Important new insights into bureaucratic aspects of foreign policy making are being developed in a current study by Morton H. Halperin, *Bureaucratic Politics and Foreign Policy,* (Washington, D.C.: Brookings Institution, March 1970 [draft manuscript]).

A number of organizational theorists have discussed the phenomenon of bureaucratic or administrative "politics" and ways in which internal conflicts over policy may be resolved or regulated. For a brief summary see Julian Feldman and Herschel E. Kanter, "Organizational Decision-Making," in *Handbook of Organizations,* ed. James G. March (Chicago: Rand McNally, 1965, pp. 638–641.)

Recent case studies of bureaucratic politics include John D. Steinbruner, "The Mind and Milieu of Policy-makers: A Case Study of the MLF," Ph.D. dissertation, Department of Political Science, Massachusetts Institute of Technology, February 1968; Richard Neustadt, *Alliance Politics,* (N.Y.: Columbia University Press, 1970); Morton H. Halperin, "The Decision to Deploy the ABM: Bureaucratic Politics in the Pentagon and White House in the Johnson Administration," paper presented at APSA meetings, Sept., 1970; Leon V. Sigal, "The 'Rational Policy' Model and the Formosa Straits Crises," *International Studies Quarterly,* 14, (June, 1970), 121–156; and Samuel R. Williamson, Jr. " 'Stumbling' Toward War: Organizational Structure and the Austro-Hungarian Experience, 1912–1914," paper delivered at the APSA annual meetings, September, 1971. Earlier case studies of "administrative" or "bureaucratic" politics are listed by Allison in his bibliography.

[30] Although most research on bureaucratic politics has confined itself to descriptive case studies and explanatory theory, often implying that little could be done to eliminate or control its worst excesses, some specialists have recently turned to a cautious exploration of ways in which the chief executive might master it. See Morton H. Halperin, "Why Bureaucrats Play Games," *Foreign Policy* (May, 1971), pp. 70–90, and particularly his recent proposal for restructuring the defense policy-making system to improve the quality of advocacy and the ability of the President to benefit from it ("The President and the Military," *Foreign Affairs,* 50 [January, 1972], 310–324). See also Graham T. Allison and Morton H. Halperin, "Bureaucratic Politics: A Paradigm and Some Policy Implications," *World Politics* (forthcoming).

[31] See, for example, Henry S. Rowen, "Bargaining and Analysis in Government," in *The Administrative Process and Democratic Theory,* ed. Louis C. Gawthrop (Boston: Houghton Mifflin Co., 1970), pp. 31–37. Similarly, William Capron offers the insightful observation that centralized management tools such as program budgeting systems, if correctly applied, can yield a more productive and effective decentralized decision-making structure ("The Impact of Analysis on Bargaining in Government," reprinted in Gawthrop, 354–371).

Since conflict over policy and advocacy are inevitable within a complex organization, one solution lies in ensuring that there will be multiple advocates within the policy-making system who, among themselves, will cover a range of interesting policy options on any given issue. Further, if a system of multiple advocacy is to function effectively, each participant must have minimal resources needed for advocacy, and certain rules of the game may be necessary to ensure proper give-and-take. A system of multiple advocacy works best and is likely to produce better decisions when three conditions are satisfied:

(1) No major maldistribution among the various actors of the following resources:

- (a) Power, weight, influence.[33]
- (b) Competence relevant to the policy issues.
- (c) Information relevant to the policy problem.
- (d) Analytical resources.
- (e) Bargaining and persuasion skills.

(2) Presidential-level participation in organizational policy making in order to monitor and regulate the workings of multiple advocacy.

(3) Time for adequate debate and give-and-take.[34]

To state the requirements for effective multiple advocacy is to realize at once how difficult it is to satisfy them.[35] It is clear, as the first of these three conditions reminds us, that *the mere existence within the policy-making system of actors holding different points of view will not guarantee adequate multisided examination of a policy issue.* Competence, information, and analytical resources bearing on the policy issue in question may be unequally distributed among these advocates. As a result, one policy option may be argued much more persuasively than another. And there is no assurance, of course, that the policy option which is *objectively* the best will be presented persuasively, for this requires that its advocate possess adequate competence, information, and analytical resources. Deprived of a persuasive presentation, that policy option may lose out to an inferior one that happens to have more effective sponsorship.

A further complication is that policy issues are often not decided solely on the basis of the intellectual merits of the competing positions put forward. Rather, the bargaining aspects of policy making may play a considerable role in shaping and determining the final decision. Hence the distribution among the actors of the other two resources listed in our theory—power (weight and influence) and bargaining skills—may be critical in shifting the decision toward one or another of the options. Thus, an option put forward by an advocate with superior competence, adequate information, and good analytical resources will not necessarily prevail over options advanced by an advocate who is less resourceful in these respects but operates with superior power and bargaining skills.

Maldistribution of resources among the various actors can take many forms. Our purpose here is not to develop predictive theories about what combinations of these five resources are most likely to enable a player or a coalition to prevail in the clash over choice of policy. Rather, we wish to help develop a theory for converting bureaucratic politics among multiple actors into a more effective and reliable system of multiple advocacy. Useful for such a theory is the working assumption that *all five resources* will be needed to some degree *by each actor or by each coalition* to perform effectively as an advocate in a system of multiple advocates.[36]

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[32] To these three conditions Hilsman appended an additional proviso: "that the circumstances make no demand for speed," and he suggested the usefulness of distinguishing among "crisis policy," "program policy," and "anticipatory policy." Roger Hilsman, "The Foreign Policy Consensus: An Interim Research Report." *Journal of Conflict Resolution,* 3 (December, 1959), 361–382. Hilsman's more recent views on improving the policy-making machinery (in *To Move a Nation,* esp. pp. 564–576, and *The Politics of Policy Making in Defense and Foreign Affairs* [N.Y.: Harper & Row, 1971], Ch. 10) restate and elaborate the ideas he presented in 1959.

[33] This includes a number of different things: access to the President or other senior officials, and the ability to use their confidence and trust as a bargaining asset; responsibility for implementation of policies decided upon, which amplifies one's voice in policy making; the ability to go outside the executive branch itself to secure powerful allies in Congress, among specialized foreign policy elites, in the media, etc.

[34] Time-compressed, stress-producing international crises are particularly likely to strain the workings of multiple advocacy even while making such advocacy more important than ever for obtaining a balanced, multisided examination of options. I shall discuss this further when I take up the various types of malfunctions the foreign policy-making process is prone to suffer. More generally, a substantial literature has accumulated on the effects of crisis on the rationality of organizational decision making. A particularly useful and current review of laboratory and field studies of the effects of stress is provided by Ole R. Holsti, *Crisis, Escalation, War.*

[35] It should be noted that the balancing of actor resources is relevant also in forming *ad hoc* policy planning groups and interdepartmental task forces. The staffing of these ad hoc groups should include not merely representatives of the different agencies but, in order to assure effective multiple advocacy, individuals with adequate weight, competence, bargaining and persuasive skills. Sensitivity to these considerations is evident also in the staffing of some of the NSC-centered interdepartmental committees in the Nixon Administration. (See, for example, Orr, "Defense Report," p. 881.)

It should be clear that the theory of multiple advocacy poses some rather sharply defined requirements for managing the policy-making system. We shall give considerable attention in the remainder of this paper to presidential-level involvement in the policy-making system—the second requirement of our theory. At this point it suffices to note three general tasks that the chief executive and his staff will have to perform to ensure that reasonably adequate forms of multiple advocacy can be created out of the vagaries of the working of bureaucratic politics. First, the chief executive may have to take steps if not to equalize resources among the advocates, then to avoid gross disparities in them. Second, the chief executive must be alert to the danger that a sufficient range of policy alternatives may not be encompassed by the advocates available for participating in the resolution of a particular policy issue: in that event, he may bring in outsiders or members of his own staff to serve as advocates for different interests or policy options. Third, he may have to develop certain rules of the game to maintain fair competition and to avoid "restraint of trade" among the advocates.

Multiple Advocacy and "Partisan Mutual Adjustment"

The model of competitive *but balanced* policy making I have outlined is more aptly designated as "multiple advocacy" than as "bargaining" or "partisan mutual adjustment." It is important to note that multiple advocacy is not a pure or "free market" bargaining game. Rather, it is a "mixed system" which requires centralized management to maintain and make use of internal competitive processes. Effective multiple advocacy does not just happen. It requires management to structure and interrelate several different roles within the policy-making system. Not all actors are cast in the role of "advocate." Two other distinct roles are also required on the part of top-level management: the role of *custodian* of the policy-making process and that of *magistrate* who presides at the apex.[37]

Before proceeding, it will be useful to note that multiple advocacy differs from Charles Lindblom's concept of "partisan mutual adjustment" in several important respects.[38] Lindblom means his propositions about "partisan mutual adjustment" to apply to the workings of the political system as a whole. He does not focus his analysis, as we do here, on decision making within the executive branch. Moreover, while multiple advocacy is a model of discussion and negotiation, "partisan mutual adjustment" is a more comprehensive model of interaction of many kinds, of which discussion with negotiation is only one.[39]

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[36] As these observations imply, the full complexity and variability of the relationship between analysis and bargaining is not adequately captured in organizational theory. The fundamental proposition advanced by James G. March and Herbert A. Simon (*Organizations,* [N.Y.: Wiley, 1965], p. 131), i.e., that rational, analytic decision-making processes take precedence over bargaining only when the actors in the system share the same operational goals, may be generally valid; but it leaves open the important question of how bargaining and analysis can best be combined or, at least, reconciled to improve policy performance. As system analysts have suggested, at the very least, analysis can usefully moderate bargaining processes and clarify for each advocate the relative utility of different possible compromises before he accepts one of them. Beyond that, a competent analytical staff reporting directly to the executive can act as an important constraint on the partisan analyses of advocates and on the bargaining among them.

[37] For assistance in elaboration of these roles, I am indebted to Scott C. Flanagan, *Aborted Democratization: The Inter-War Crisis in Japan,* draft Ph.D. dissertation, Political Science Department, Stanford University, 1971.

[38] Lindblom's earlier views on the advantages of the bargaining model appear in his "Bargaining: The Hidden Hand in Government," The RAND Corporation, RM-1436-RC (22 February, 1955). A substantial broadening and fuller exposition of the bargaining model, which Lindblom renamed "partisan mutual adjustment," appears in *The Intelligence of Democracy: Decision-Making Through Mutual Adjustment* (N.Y.: Free Press, 1965). Interesting refinements and additions appear in his most recent statement, *The Policy-Making Process* (Englewood Cliffs, N.J.: Prentice-Hall, 1968).

[39] This point was made by Charles Lindblom in a personal communication (October 12, 1971); he adds that "in some forms of partisan mutual adjustment, to be specific, the mutual adjustment is achieved by mutually adaptive moves of distant parties who do not in any way communicate with each other and indeed may not even know each other or be able to identify each other." In any form of mutual adjustment, "the adjustments are made serially by persons or parties that react to preceding moves of other parties and are in turn reacted to by successive moves of further parties—all in circumstances in which no person or party sees the earlier moves of the other

A comparison between "partisan mutual adjustment" and multiple advocacy, therefore, will be inappropriate and misleading unless these differences in scope and focus are kept in mind. Nonetheless, we may note a tendency in the discussion of "partisan mutual adjustment" and in Lindblom's earlier essay on "Bargaining: The Hidden Hand in Government," which was more directly concerned with the executive branch, to assume that various factors will operate naturally to induce effective coordination in the absence of centralized control or intervention. The logic of "partisan mutual adjustment" implies not merely automaticity and inevitability of coordination; it also suggests that the policy choice that emerges will be about as good as can be obtained, given the complexity of the issues and the variety of interests activated by them.[40]

In contrast, the multiple advocacy model makes explicit the assumption that effective competition is usually necessary if the processes of bureaucratic politics within a complex organization are to produce the functional equivalent of a rational consideration of, and choice among, policy alternatives. We noted earlier several dysfunctional aspects of bureaucratic politics ("quasi-resolution of conflict" and "negotiating the internal environment") which are, in fact, examples of imperfect competition in pluralistic systems. We also noted that the Nixon administration has resorted to a centrally directed, depoliticized system of policy making in order, among other reasons, to avoid dysfunctional consequences of this kind. The alternative examined here, multiple advocacy, does not ignore the dysfunctional workings of bureaucratic politics. It attempts to deal with them, however, by utilizing the resources and involvement of the top-level executive and his staff in a different fashion than does the presently constituted NSC system.

In contrast to "partisan mutual adjustment," therefore, multiple advocacy is a *management-oriented* theory. It warns that the presumably beneficial effect of internal competition and conflict on organizational policies does not obtain if the competitors are unevenly matched, or if they divide up the market, engage in unfair competition, squeeze out or buy off weaker competitors, etc.[41] Accordingly, multiple advocacy assigns to top-level authority in the organization the task of maintaining and supervising the competitive nature of policy making within the organization. This task belongs to the *custodian* role, which we have already mentioned and to which I shall give considerable attention in the remainder of this paper. The custodian's role can be introduced and maintained only by the chief executive. (It should be noted that the highly centralized policy-making model of the Nixon Administration also requires a custodian; some of his tasks are similar to those of the custodian in a multiple advocacy system.) In a multiple advocacy system the chief executive must define his own role as that of a *magistrate* who evaluates, judges, and chooses among the various policy options articulated by advocates. The central importance of the magistrate role should not be overlooked or taken for granted. It is only because a magistrate presides at the apex of the policy-making system that multiple advocacy can be maintained and regulated. The introduction of the magistrate role into the system means that advocates are no longer competing against each other (as would be the case in a decentralized bargaining system such as "partisan mutual adjustment"); rather, the advocates are competing for the magistrate's attention. The custodian can influence such competition even if the resources of the advocates are unbalanced; in fact, it is particularly necessary for him to do

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parties as anything but part of the environment to which he is reacting and in which it may not be at all conspicuous." Types of partisan mutual adjustment and the position of negotiation therein are indicated in Lindblom's *Intelligence of Democracy,* pp. 33–4.

[40] For a incisive critique of these and other assumptions of pluralist theory, see Allen Schick, "Systems Politics and Systems Budgeting," *Public Administration Review,* 29 (March-April 1969), 139–150.

[41] It is to precisely these dangers that proponents of centralized management tools such as systems analysis and program budgeting have called attention in diagnosing the deficiencies and risks of pure bargaining models of policy making. Thus, as Henry Rowen "Bargaining and Analysis," p. 35 has observed: "There are not only wide differences in the bargaining power of the 'firms' (bureaucratic policy actors), this bargaining power is not necessarily highly correlated with the information or the power to take relevant action to accomplish objectives with a high degree of efficiency."

On occasion Lindblom himself has briefly acknowledged the possible deficiencies and poor performance of the bargaining model. For example, in "Bargaining: The Hidden Hand in Government," p. 24, he acknowledged that "carried to decision-making processes in which a strong hierarchical element is desirable, bargaining in the wrong place at the wrong time accounts for some of the worst aspects of American government." He has had little to say, however, about how to avoid bad variants and ensure good variants of the bargaining model. More recently, in *The Policy-Making Process,* pp. 33–4, while continuing to note the limits of policy analysis, Lindblom recognizes the contributions that even "partisan analysis" can make to clarification and improvement of policy.

so when there is a marked disparity in bargaining advantages among advocates.

As this implies, in his role as magistrate, the chief executive does not simply decide in favor of the strongest coalition of advocates. Rather, his central position and ultimate responsibility give him the obligation to evaluate the relative merits of competing positions and the power to decide against the majority. In order to discharge this responsibility, the chief executive will continue to require a strong analytically-oriented staff such as that of the NSC.

It is important to recognize that the responsibilities of the chief executive for policy making are not necessarily confined to the role of magistrate, which *he* exercises, and the role of custodian, which he would be wise to delegate to a senior assistant. In addition, from time to time the chief executive will find it desirable to initiate policy advocacy from the presidential level, particularly when departmental officials do not become advocates for policy options that deserve serious consideration, either because they do not attach high enough priority to such options or perceive departmental disadvantages in them. In such instances, the chief executive may play the advocate's role himself or encourage policy advisers on his staff to exercise the prerogatives of advocates. In the latter event, I shall argue, the chief executive should *not* ask the person whom he has charged with the custodian's role to act also as adviser-advocate.

As the preceding discussion implies, the chief executive's personal orientation to manifestations of "politics" and conflict is likely to be particularly important in determining the ways in which he will attempt to cope with bureaucratic politics and the extent to which it can be converted into an effective system of multiple advocacy.[42]

Before proceeding we must take note of another factor that affects the ability of the chief executive to play the roles we have assigned to him in our theory of multiple advocacy. In the American system of government, as Richard Neustadt pointed out so incisively in his *Presidential Power* (1960), the President must be an ever-wary, active and skillful participant in the arena of bureaucratic politics if he is to protect his own interests. Neustadt defined these interests largely in terms of the President's *personal power position.* As Neustadt emphasized, presidential power is not an ample and stable commodity which the incumbent receives upon entering office and which is available to him ever thereafter to be used when needed. Rather, presidential power is a highly unstable variable whose magnitude can vary greatly depending upon the incumbent's resourcefulness as a political entrepreneur and his skillfulness at the game of bureaucratic politics.

We may note that Neustadt did not attempt to develop a broader theory for strengthening multiple advocacy. He confined himself, rather, to various kinds of advice on how the president might maintain his personal power position and utilize it effectively.[43] In fact, it must be noted that Neustadt focused his advice to presidents somewhat too narrowly on the problem of protecting their personal power positions.[44] He did not address the broader question of what the President can do to ensure that better public policy decisions will emerge from the inevitable play of bureaucratic politics. There is indeed an underlying assumption in parts of Neustadt's book—which in fact may be the major theoretical proposition he wishes to advance—that if the President succeeds in protecting his personal power stakes in controversial policy matters, he will thereby also ensure better policy decisions for the country at large. This is sometimes true, but the exceptions can be extremely important. A chief executive may resist initial indications that a change of policy is needed because he feels, and is advised, that to do so will damage his prestige and reputation. But by protecting his personal power stakes too assiduously the President may become gradually locked into a disastrous policy. I shall return later to the possibilities for genuine role conflict between the executive's interest in protecting his personal power stakes in a policy issue and his broader responsibility for involving himself in the policy-forming process in order to secure better advice and better decisions.

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[42] I have discussed various orientations to "politics" and political conflict held by earlier presidents and the strategies they employed to cope with bureaucratic politics in the executive branch in "Stress Adaptation in Political Decision-making."

[43] The advice Neustadt's book offers a president by no means exhausts the possibilities available to him. In a cogent explication and critical appreciation of *Presidential Power,* Peter Sperlich argues that Neustadt overstates the President's need to rely exclusively on bargaining and persuasion to influence others and notes that the chief executive has other sources of authority and influence, such as organizational ideology and norms, at his disposal for this purpose. Sperlich also calls attention to the danger of "overload and breakdown" if a president attempts to follow Neustadt's advice too literally. (Sperlich, "Bargaining and Overload: An Essay on *Presidential Power,*" in *The Presidency,* ed. Aaron Wildavsky, [Boston: Little, Brown & Co., 1969], pp. 168–192.) On the chief executive's ability to use symbolic manipulation, not merely commands, to influence subordinates and other actors in the political system, see also Murray Edelman, *The Symbolic Uses of Politics* (Urbana: University of Illinois Press, 1964).

[44] This has inevitably made some political scientists uneasy and led to the view that *Presidential Power* is too Machievellian. A useful commentary along these lines is provided by William T. Bluhm, *Theories of the Political System* (Englewood Cliffs, N.J.: Prentice-Hall, 1965).

President Kennedy himself is reported to have been made uneasy at suggestions that he was modeling his presidency along the lines of Neustadt's prescriptions. See Arthur M. Schlesinger, Jr. *A Thousand Days* (Boston: Houghton Mifflin Co., 1965), pp. 678–9; Theodore C. Sorensen, *Kennedy* (N.Y.: Harper & Row, 1965), p. 389.

More generally, faith in a strong president and in the virtues of presidential power has been called into question by a number of political scientists in recent years. See Thomas E. Cronin, "The Textbook Presidency and Political Science," paper, delivered at American Political Science Association meetings, September, 1970; and William G. Andrews, "The Presidency, Congress, and Constitutional Theory," paper, presented at American Political Science Association annual meetings, September, 1971.

The "Collegial" Style of Decision Making

I have emphasized thus far the management tasks of balancing actor resources and maintaining a competitive relation among advocates. However important to the workings of the multiple advocacy system, these tasks do not deal with all the threats to rational decision making generated by the bureaucratic organization of functional expertise; nor, fortunately, do they exhaust the coping devices available to the chief executive. I shall now discuss another strategy available to him for strengthening multiple advocacy by ameliorating the narrowness of perspective and rigidity with which officials in departments and agencies often play the game of bureaucratic politics. Thus, a president may attempt to *restructure and redefine the roles* of top-level "advocates" from the different agencies and to *resocialize them into a new set of decision-making norms.* The objective in so doing is to broaden the perspective with which these officials view policy problems without, however, requiring them to abandon the valuable identification with agency viewpoints and functional expertise.

This strategy is particularly appropriate as a way of dealing with novel situations in which important values are at stake but which cannot, or should not, be dealt with by selecting one of the standard responses from the organization's available repertoire. To develop policy that can cope adequately with challenging situations of this kind, it is often helpful to rearrange the milieu and norms of policy-making personnel so that they are able to ask questions that do not hue closely to the dominant ideology and policy doctrines of the organization as a whole or those of its subunits. This flexibility is much more difficult to achieve when the quest for an effective decision proceeds within the context of a formalized bureaucratic process of policy making. For various reasons a more loosely structured and informal milieu—i.e., the "collegial" style of decision making—makes it somewhat easier for most participants to free themselves and others from the inhibitions of organizational doctrines and from the tendency to overprotect the special interests of the subunit and constituency with which they are identified. Particularly because these inhibitions tend to be implicit and deeply ingrained, they can easily stifle novel ways of looking at policy problems.

The possibility for fruitfully restructuring roles and norms toward this end was displayed by President Kennedy in the Cuban missile crisis. The lessons he and his close advisers had drawn from their inept policy making in the Bay of Pigs case were put to use in improvising a quite different approach to crisis decision making when they were suddenly confronted in October, 1962 by Khrushchev's deployment of missiles into Cuba. Kennedy's inability to eliminate all dysfunctional manifestations of bureaucratic politics during the Cuban missile crisis[45] does not reduce the value of the case for illustrating the major features and rationale of the collegial mode of policy making.

Following the discovery that a substantial number of surface-to-surface missiles of medium and intermediate range were being emplaced in Cuba, many questions arose for U.S. policy that required integrated evaluation from political, diplomatic, and military standpoints. The President immediately created a special *ad hoc* group of advisers from different branches of the government to consider the implications of the missile deployment for U.S. security interests and to identify, develop, and evaluate alternative courses of action for his consideration. This group came to be called the Executive Committee (ExCom) of the National Security Council.

As has been emphasized in all available accounts, Kennedy quickly established a *modus operandi* and ground rules for the deliberations of the ExCom that greatly facilitated performance of the critical "search" and "evaluation" phases of policy analysis before the *final* "choice" was made. The emphasis on "final" here calls attention to the fact that search and evaluation were not separated bureaucratically or sequentially from the task of choice. Those who engaged in search and evaluation also participated in thrashing out the choice; and an iterative approach to problem solving was employed, so that rehearsal and debate over choice was allowed to have a feedback for renewed search and evaluation. Moreover, the inept leadership practices Kennedy had displayed in managing the policy discussions that led to his Bay of Pigs decision were replaced by a much more sophisticated set of techniques for conducting policy discussions on what to do about the missiles in Cuba.[46]

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[45] On this point see Allison, *Essence of Decision,* Chs. 4 and 5.

In the Cuban missile crisis the ExCom was given the character of an informal problem-solving group that was to concern itself with all relevant aspects of the problem. Of central importance was the encouragement of each member of the group to concern himself with the policy problem as a whole rather than to confine himself to that element of it—be it intelligence, military strategy, international affairs, diplomacy—on which he was an expert or for which he or the office to which he belonged was officially responsible. This mode of policy deliberation enhances the role of the "generalist" and gives him more scope for interacting with specialists and challenging their expert opinions.[47]

As the detailed accounts particularly by Sorensen and Abel indicate, the meetings of the ExCom were marked by considerable give and take.[48] Protocol was suspended. Second-level officials were encouraged to give their views even when they were at variance with those of superiors who were present. The President was encouraged by some of his close associates to absent himself from some of the meetings when it was found that the process of mutual exploration of views was freer and more productive when he was not present. The meetings were noteworthy for the instrumental use made of advocacy and role playing as means of exploring and illuminating the critical issues. Most participants are reported to have changed their positions at least once during the course of the six days prior to the President's final decision on a course of action. The President himself defined his own role as closely resembling that of the "magistrate." He listened to well-prepared advocates make the best case for and against the blockade and the air strike, and asked questions before making his final decision.

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[46] In *Problem-Solving Discussions and Conferences: Leadership Methods and Skills* (San Francisco: McGraw-Hill, 1963), N. R. F. Maier outlines nine basic principles for leading a problem-solving group in order to enhance the quality of its "solution" or decision. Although employing a terminology that differs somewhat from that employed by organizational theorists, Maier's principles can be translated into the multifunction decision framework of "search"-"evaluation"-"choice," and many of his principles are applicable to the management of small political decision-making groups. He emphasizes, for example, the value of (a) turning situations that are prematurely structured for "choice" among immediately obvious options into a fuller discussion of the "problem"; (b) preventing the "search" for options to be inhibited by too early or too dominant a preoccupation with "evaluation"; (c) requiring the group to obtain a "second solution" (i.e., an iterative approach to problem solving) to encourage issues to be raised that were previously overlooked or suppressed; (d) encouraging and protecting the expression of minority opinions, and utilizing disagreement to create the conditions for creative problem solving; etc.

In separate seminar papers, two of my students, Mike Garner and Craig Cotora, applied Maier's principles in order to compare and evaluate Kennedy's management of the policy-making process in the Bay of Pigs and Cuban missile crises. The results indicate that Kennedy "violated" most of Maier's principles in making the Bay of Pigs decision while acting in consonance with them in the Cuban missile case. A particularly incisive comparison of Kennedy's handling of top-level policy deliberations in the two cases, with reference to small-group dynamics that affect the performance of advisory groups, is presented in Irving Janis, *Victims of Groupthink* (forthcoming).

[47] Although the importance of the "generalist" has often been emphasized by experienced policy makers, the role does not appear to be well defined. Also, it seems to have received little formal attention in studies of group problem solving. As Victor Thompson suggests, one should not restrict one's view of the ideal generalist to a person who is broad-gauged without being, or having been, an expert of some kind. Rather, "generalist" should be thought of as a special kind of relationship—that is, a role that even the expert can play vis-à-vis subjects on which others in the group, not he, are specialists. But for an expert to play the role of generalist he must be willing to violate the tacit agreement, often entered into by experts, not to challenge each other's expertise. The essence of the generalist's role is to ask the questions his very ignorance, naivete and different perspective allow him to ask of experts, questions which might never occur to the experts or whose relevance would not be evident to them until pointed out. (Thompson, *Bureaucracy and Innovation* [University, Alabama: University of Alabama Press, 1969], pp. 12–14; see also *Specialists and Generalists: A Selection of Readings*, U.S. Congress, Senate, Subcommittee on National Security and International Operations of the Committee on Government Operations [Washington, D.C., Government Printing Office, 1968].) For various reasons I question the assumption that the role of "generalist" in problem-solving groups can be played adequately by the political leader or chief decision maker.

[48] These remarks about the "collegial" style that dominated the deliberations of the ExCom draw on Theodore C. Sorensen, *Kennedy*, pp. 679–680, 684–686; and Elie Abel, *The Cuban Missile Crisis*, pp. 47, 52, 69–73, 79, 86–88. Similar observations are to be found in other sources on the Cuban missile crisis. See also my case study of the Cuban missile crisis in A. L. George, David K. Hall, and William E. Simons, *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam* (Boston: Little, Brown & Company, 1971).

The availability of time before the President would have to act proved to be highly useful. It permitted the ExCom to go through the process of problem solving several times. This iterative approach facilitated mutual education and made it possible for the group progressively to narrow down and clarify the critical factors on which the final choice of action depended.

The ExCom did not overlook the necessity for obtaining specialized inputs from experts on political, military and diplomatic questions; indeed, its iterative approach to policy analysis permitted the group to obtain an increasingly sharper definition of critical questions requiring more data and specialized study. In this respect, the ExCom acted as an informal steering group for bringing to bear the best knowledge and analytical capabilities located elsewhere, at lower and more specialized branches of the government. But perhaps the major contribution the collegial mode of deliberations in the ExCom made to effective policy making was to maintain an atmosphere in which all relevant political, military, intelligence, and diplomatic considerations could be identified, and the relationships among them kept in mind in devising and assessing alternative courses of action.

A collegial mode of policy making at the apex of the organization offers important advantages over policy-forming models in which participants reflect more closely the characteristic organizational features of hierarchy, specialization, and centralization. In the ExCom case the collegial model probably operated at something approaching optimal performance in a crisis for special reasons that may not always be present. Nonetheless, it would be useful to understand better the special factors that contributed to the ExCom's performance in order to judge whether and how they might be replicated on future occasions.[49] To sharpen appreciation of the most advantageous features of collegial policy making in crises let us compare this model with an extremely bad variant of the formal bureaucratic model that deliberately caricatures it. In this extreme hypothetical variant of the bureaucratic model, planning and decision making are highly structured, expertise is compartmentalized along narrow functional lines, and the system operates in a highly formal manner. Information and judgment on each relevant element of a current complex policy problem are provided to the chief executive solely by the recognized functional expert on that matter. Participants define their organizational roles narrowly and perform them inflexibly. No use is made of the "devil's advocate." Furthermore, the chief executive makes no provision for having functional experts on specialized aspects of the problem engage in a genuine exchange with each other in order to explore the policy problem as a whole and to examine the interrelationships among its different elements. A formal procedure is followed for obtaining each expert's judgment on his portion of the problem—a judgment, it is expected, that others are not competent to question. Each actor may also be asked for his overall judgment, but he gives it without having understood or studied the problem as a whole. Since there is little or no use of an iterative approach to problem solving in this particular bureaucratic policy model, the experts have no opportunity to revise their judgments. Further, the highly structured, compartmentalized approach to policy making casts each adviser into the role of spokesman for his group. This forces the participants to resort to bargaining and superficial compromises rather than to utilize analytic procedures as a means of dealing with disagreements over policy.

This variant of the bureaucratic model is indeed a poor way of meeting perhaps the most critical task in problem solving, namely, analysis of the relationship of the various parts to the whole. When a "narrow, functional" bureaucratic approach is employed for this purpose, the heavy if indeed not impossible intellectual burden of synthesizing the many elements of the problem is placed upon the chief executive and his immediate personal staff. In contrast, the collegial mode is a special form of multiple advocacy. In addition to balancing actor resources and maintaining a competitive relation among advocates, it strengthens the analytic component of policy making and reduces the influence of the bargaining component. It does so by restructuring the roles of special advocates, enhancing the role of the "generalist" vis-à-vis functional experts, and introducing new norms governing the process of deliberation.

"Malfunctions" of the Policy-Making Process and the Need for Preventive Interventions

Despite numerous efforts in the past to restructure the machinery and procedures of foreign policy making,[50] it is probably not unfair to say that improvements in policy performance have been marginal and uneven. While structural reorganization can aid in the quest for effective decision making, there appears to be no single structural formula by means of which the chief executive and his staff can convert the functional expertise and diversity of viewpoints of the many offices concerned with international affairs into consistently effective policies and decisions. This sober observation coincides with the evaluation of a broader range of experience in many different kinds of organizations. The optimistic view that principles of modern organization and administration, and indeed bureaucracy itself, provided a vehicle for achieving greater rationality in the conduct of social and governmental affairs has long since faded. As one specialist on organization, Warren Bennis, has put it: "the pyramidal, centralized, functionally specialized, impersonal mechanism known as bureaucracy—was out of joint with contemporary realities." Continuing, Bennis predicts that "adaptive, problem-solving temporary systems of diverse specialists, linked together by coordinating executives in an organic flux—this is the organizational form that will gradually replace bureaucracy."[51] While the outlines of this vision remain dim, it is clear that theorists of administration and organization are breaking new ground once again and that a period of exploration lies ahead. The theory of effective multiple advocacy and the related model of collegial (as against bureaucratic) decision making just described are very much in accord with novel road maps that organizational theorists are beginning to chart.

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[49] Less pronounced forms of the collegial model of policy discussions are also cultivated in hierarchical systems. Robert H. Johnson ("National Security Council," p. 727) describes efforts to achieve some of the advantages of this approach within the NSC system under Eisenhower.

In arguing for a new, more flexible form of organization, Bennis recognizes the paradox that even highly flexible systems need to be managed in appropriate ways. "A question left unanswered," he notes, "has to do with leadership. How would these new organizations be managed?"[52] We face a similar problem here, one whose difficulty must be acknowledged. I have given some indication of how a collegial system of decision making can be created and managed within a small advisory group at the presidential level—and, in fact, at lower levels of decision making as well. But there are additional tasks of managing policy making in complex organizations such as the government. Experience indicates that any system for policy making, however well designed, is subject to periodic failures and gross malfunctions. As Harold Wilensky has emphasized, all large-scale organizations have structural characteristics of hierarchy, specialization, and centralization that encourage chronic pathologies of information and advice.[53] As a result, organizational theorists have increasingly emphasized that the design of machinery and operating procedures for policy making must take into account the types of human failures, individual and group dynamics, organizational pathologies and malfunctions that can occur.

A sophisticated approach for designing a better policy-making system, therefore, will not be content with restructuring the organization and rewriting its standard operating procedures; it will also include mechanisms for timely identification and correction of possible malfunctions.[54]

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[50] A selective bibliography appears in Clark and Legere, *President and Management of National Security*.

[51] Warren G. Bennis, "Post-Bureaucratic Leadership," *Transaction* (July-August 1969), pp. 44–61. See also W. G. Bennis and Philip E. Slater, *The Temporary Society* (N.Y.: Harper & Row, 1968); and Alvin Toffler, *Future Shock* (N.Y.: Random House, 1970), Ch. 7, "Organizations: The Coming Ad-hocracy."

Charles Perrow's concept of "multiple leadership" is similar in some ways to the notion of policy making via multiple advocacy (Perrow, "The Analysis of Goals in Complex Organizations," *American Sociological Review,* 26 [December, 1961], 854–866).

[52] Bennis, "Post-Bureaucratic Leadership," p. 45. Bennis recognizes that one task will be the control of conflict within such organizational forms. It will be necessary, he believes, to develop ways of developing "group synergy" and "collaborative cultures." The new concept of leadership required will include (what we also stress here) "practical theories of intervening and guiding these systems, theories that encompass methods for seeding, nurturing, and integrating individuals and groups" (p. 51).

[53] Harold Wilensky, *Organizational Intelligence* (N.Y.: Basic Books, 1967). For a more general discussion of six types of "failures or pathologies" to which every political decision system is prone, see Karl W. Deutsch, *The Nerves of Government* (N.Y.: Free Press, 1963), Ch. 13, "The Self-Closure of Political Systems." Breakdowns in presidential advisory systems are discussed and illustrated by Neustadt, *Presidential Power,* and by Aaron Wildavsky, *Dixon-Yates: A Study in Power Politics,* (New Haven: Yale University Press, 1962).

[54] Thus, for example, Wilensky, pp. 174–181, lists a number of "organizational defenses against informational pathologies," and Anthony Downs identifies strategies (such as redundancy, use of counter-biases, eliminating the middlemen, developing distortion-proof messages) for reducing or avoiding distortion in hierarchical communication within the organization. (*Inside Bureaucracy* [Boston: Little, Brown, 1967], pp. 118–127). See also Davis B. Bobrow, "International Indicators," paper presented at annual meeting of the American Political Science Association, September, 1969; and Eugene J. Webb, "Individual and Organizational Forces Influencing the Interpretation of Indicators," (unpublished ms.) Stanford University, April, 1969.

Provision must be made for monitoring the day-to-day workings of the policy system, and strategies for "preventive intervention" must be available within the policy-making system. Clearly the chief executive has unique opportunities as well as responsibilities in this respect. "Preventive intervention" is not a novel concept. In trying to adapt and incorporate it into foreign policy making, one should draw upon experience gained in other policy arenas. Stimulating analogies are also available in biological and cybernetic models of "self-organizing systems." In John von Neumann's phrase, self-organizing systems "contain the necessary arrangements to diagnose errors as they occur, to readjust the organism so as to minimize the effects of errors, and finally to correct or block permanently the faulty component." Such systems are "able to operate even when malfunctions set in . . . while their subsequent tendency is to remove them."[55]

The strategy I have chosen for developing this kind of pragmatic management theory consists of diagnosing past policy making within the government with the object of inductively codifying the lessons of experience. This approach is favored over one that would rely on more abstract preconceptions or theoretical models about how complex organizations can be made to engage in more "rational" decision making. Such an inductive approach, it should be noted, gives our prescriptive model a strong empirical base. (At the same time, however, formal models of rational decision making can have heuristic value for diagnosing past performance and for setting general goals to be furthered by preventive interventions.)

In order to identify malfunctions to which the U.S. foreign policy-making system is prone, I will now examine the workings of the policy-making process in a number of recent historical cases involving a decision to make a commitment, to intervene in, or to escalate a conflict.[56] The cases to be examined are: the North Korean attack on South Korea in late June 1950; the Chinese Communist intervention in Korea in the Autumn of 1950; the Dienbienphu crisis in Indochina of 1954; the Bay of Pigs incident in early 1961; the increase of U.S. military advisers in South Vietnam in late 1961; Johnson's decision with regard to the Multilateral Force for NATO in October 1964; the Vietnam escalation of February-March 1965; and the Dominican intervention in 1965 (as well as the Cuban missile crisis already discussed).

Beyond the attempt to choose "critical" decisions on matters of substantial import which, once made, could not easily be reversed, no particular sampling strategy was employed other than to pick recent cases on which historical materials are perhaps adequate for the purpose at hand. (The selection of these cases was also influenced by my greater familiarity with some recent cases and preliminary hunches about their didactic value.) A larger and broader variety of cases could be profitably examined. Working with relatively few cases is acceptable insofar as the research objective is to develop a typology of malfunctions rather than to assess the likelihood or frequency of any given malfunction, or to develop generalizations based on a representative sample of a given universe or class of foreign policy decisions.

Certainly, since the number of cases examined is limited, there is no assurance that the typology of malfunctions presented here is complete. Study of other cases may identify still other types of malfunctions.[57] The inability to assess the expected frequency of different malfunctions is not necessarily a handicap at this stage in the construction of a prescriptive theory for managing the policy-making process. Insofar as a malfunction could have severely adverse consequences, it is prudent to design the policy-making system, if possible, at acceptable cost, to reduce the likelihood of its occurrence *even though* that type of malfunction is not expected to occur frequently.

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[55] John von Neumann, "The General and Logical Theory of Automata," in *The World of Mathematics*, ed. James R. Newman (N.Y.: Simon and Schuster, 1956). Vol. IV, 2085–2086. (Quoted by Martin Landau, in "Redundancy, Rationality, and the Problem of Duplication and Overlap," *Public Administration Review*, 29 [July-August, 1969], 346–358.) Stimulated by mathematical demonstrations that redundancy can help create a system that is more reliable than any of its parts, Landau argues that duplication and overlap in administrative agencies are not necessarily signs of waste and inefficiency, though he admits the task remains to distinguish between inefficient redundancies and those that are constructive and reinforcing.

[56] The empirical base for this part of the multiple advocacy theory is, therefore, admittedly confined to a special (though very important) class of foreign policy decisions. A broader empirical base would be desirable in order to define more clearly the scope of this prescriptive theory and to identify the types of policy problems and conditions under which it is most applicable.

[57] It should be clear that the study is concerned with possible malfunctions of the policy-making process, and not other kinds of "errors" or "flaws," such as psychological aberrations, man-machine errors, etc. To this end, I have tried to confine my definition of "malfunctions" to instances in which the policy-making process failed to replicate one or another component of the rational model of decision making, i.e., did not succeed in reproducing fully within the organizational process the functional equivalent of the formal rational model.

Neither does the present study attempt to present in any systematic way an *explanation* for a particular malfunction as does, for example, Irving Janis's theory of "groupthink," which he applies to some of the same historical cases.

Three questions were asked in diagnosing each case. The first (and easiest to judge) was whether or not a substantial malfunction of any kind occurred. The major difficulty encountered in making such determinations has to do with the adequacy of historical materials. Is the historical record full enough and reliable enough to permit such an inference? Is it highly probable or merely plausible that the malfunction in question occurred?

The second question asked of each historical case offers greater difficulty. We would like to know whether the malfunction that occurred in the policy-making process influenced in any way the content of the foreign policy decision. Might the decision have turned out otherwise had the malfunction not occurred? How important was the malfunction as compared with other variables in shaping the choice of one policy option as against another? In attempting to deal with this second question one encounters the difficulty, alluded to at the beginning of this paper, of making a discriminating causal interpretation of the many variables that operate in shaping a foreign policy decision. While employing the historian's method of causal imputation for this purpose, I have a different objective. Unlike the historian who must attempt to arrive at a definitive explanation, such high-confidence explanations are not necessary when using historical cases to develop the kind of theory of interest here. Plausible (as against definitive) interpretations of the importance of malfunctions are useful and sufficient for developing a pragmatic theory for managing the policy-making system.

One finds that in some historical cases certain ideological values, "operational code" beliefs, or other cognitive beliefs (all of which, following Herbert Simon, I shall refer to for purposes of convenience as *"decisional premises"*) are evidently so uniform and unquestioned among policy makers as to determine their choice of policy independently of whether the policy-making process worked well or badly.[58] In other historical cases, however, the decisional premises of policy makers were evidently not so coherent or homogeneous. It is particularly in these more ambiguous situations that the process of policy making may itself have considerable causal and instrumental importance if it succeeds in subjecting decisional premises and initial perceptions to critical examination and debate before policy preferences solidify behind one option.

The third question is, How effective was the decision? Even more difficult methodological problems arise in answering this question, which requires that we evaluate foreign policy decisions according to some normative criterion. The problems of normative analysis are well known, and in venturing into this area I make no claim to have solved them. It is of some help, however, to distinguish among several different kinds of normative analysis of foreign policy decisions and to consider their interrelationship. A familiar but rather circumscribed type of evaluation takes the values and objectives of the policy makers as given and considers only the "efficiency" of their policy choice for furthering those values and objectives. In making such a judgment about historical cases the investigator must confine himself to considering whether it was the best decision under the circumstances that prevailed at the time. Otherwise, the quality of the decision is scored unfairly on the basis of hindsight derived by knowledge of the outcome[59] or on the basis of new information about the initial situation not available to policy makers at the time. There are at least some explicit criteria for making this kind of evaluation: (1) the attainability of the policy goals chosen; (2) the suitability of the means employed; (3) the timeliness and flexibility of the response; (4) the accuracy of calculated support for the policy chosen; (5) the accuracy of relative estimates of one's own capabilities vis-à-vis the opponent's capabilities and intentions; (6) the accuracy of predicted long-term consequences.[60]

Assessing the "efficiency" or "effectiveness"[61] of decisions is not without relevance, but a broader normative framework is necessary for evaluating major foreign policy decisions such as those reflected in the historical cases examined here. At the most fundamental level lies the question of the *values* which guided policy makers in making these decisions. Normative analysis at this level, however, cannot be reduced to the simple question of whether the investigator agrees with the political values and objectives of the policy maker. Most policy makers adhere to a hierarchy of multiple values. Further, they often act on the assumption that the pursuit of secondary values will serve more fundamental ones. In addition, some policy means or strategies may become doctrinal beliefs which, in turn, become confused with more fundamental values. Not only does the policy maker often fail to perceive the full range of his own values that is actually engaged by a situation but he is also often insufficiently aware of the latent value conflict posed for him by the situation.

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[58] Jeffrey Race has examined a number of psychological and organizational processes that seem to lead American policy makers to systematically suppress certain ways of thinking about revolutionary movements. (In "American Intervention Abroad: Systematic Distortions in the Policy-Making Process," paper given at American Political Science Association annual meetings, September, 1970.)

[59] Relevant here is the familiar distinction in political science between "decisional output" and "policy outcome." The latter is usually determined not merely by the quality of the policy decision made earlier (i.e., the "decisional output") but by other factors and subsequent events, some of which could probably not have been anticipated by policy makers in reaching their decision.

[60] A useful discussion is provided by Glenn D. Paige, *The Korean Decision*, (N.Y.: Free Press, 1968); Ch. 12, "Evaluation."

[61] For a useful discussion of "effectiveness," which is somewhat broader than the criterion of "efficiency," see N. R. F. Maier, *Problem Solving and Creativity*, Ch. 1; Vroom, "Industrial Social Psychology," p.

Under these circumstances the very notion of "efficient" means becomes opaque and grossly inadequate for any form of normative analysis. Accordingly, a prescriptive model for policy making should not be judged solely with respect to its expected advantages for producing "efficient" decisions. It should be judged also with respect to the contribution it can make to identifying for policy makers the full range of values engaged by a decision that has to be made, clarifying their value hierarchies, dealing with conflicting values in a reasonable way, and subjecting unduly valued doctrinal beliefs and other decisional premises to critical examination.

It is often assumed that a rational model of decision making is applicable only for the more circumscribed type of normative evaluation associated with "efficiency." There are circumstances, however, in which the rational model is highly relevant also for the broader and more challenging normative task discussed here. Cost-benefit analysis can have indirect payoffs of this kind if it is conducted within a policy-making system such as multiple advocacy that gives scope for diversity and utilizes disagreements within the group to identify and examine critical decisional premises that are constraining choice from among a broader set of alternatives. Thus, sober cost-benefit analysis of a preferred option—e.g., intervention abroad—may reveal that the costs of pursuing what appears to be a dominant value may jeopardize other values of equal or greater importance. The initial determination of value priorities in a situation by the policy maker is often not subjected to adequate cost-benefit analysis before major foreign policy decisions are made. The value priorities and goals he favors in that situation can change dramatically if analysis indicates that the costs and risks of proceeding in this direction are likely to be excessive. As Samuel Johnson put it: "There is nothing like the prospect of hanging in a fortnight to clear the mind."

As I envisage it, therefore, and will attempt to suggest in the analysis of the historical cases, malfunctions of the policy-making process can have broad normative implications for the quality of foreign policy decisions. The value of "preventive interventions" to avoid these malfunctions is not necessarily limited to improving the "efficiency" of policy means.

Malfunctions of the Policy-Making System: A Typology

Nine possible malfunctions of the policy-making process have been identified. These malfunctions can occur whether the policy-making system is modeled on one prescriptive theory or another, or even when it lacks any well-defined modus operandi. "Preventive interventions" by the custodian-manager of the policy-making system, therefore, are required to deal with emerging malfunctions of this kind in a system such as the present Nixon-Kissinger National Security Council as well as in a system of multiple advocacy. The nine malfunctions thus far identified are listed separately, although they may be interrelated in a particular historical case such as the Bay of Pigs.

1. *When the President and his advisers agree too readily on the nature of the problem facing them and on a response to it.*

Paradoxically, while it is often difficult for an executive to achieve sufficient consensus within the decision-making group on behalf of

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229; and, particularly, Victor H. Vroom and Philip W. Yetton, "Normative and Descriptive Models of Participation in Decision Making," paper prepared for the Joint U.S.-Soviet seminar on Organization Design, Kiev, USSR, June 1971. Common to these discussions is the operationalization of "effectiveness" in terms of (1) the objective *quality* or rationality of the decision; (2) the *acceptance* of the decision by subordinates who will have to execute it; (3) the *amount of time* required to make the decision. A conflict or latent tension between quality and acceptance is recognized, as is the desirability of resolving it in different ways, depending on circumstances. Thus, particularly germane to this paper is Vroom's recognition, with which I agree, that "quality will be weighted heavily in strategic decisions which are not reversible . . . ," p. 239.

a wise policy without diluting its ingredients, to achieve consensus too quickly and too easily is also likely to degrade the quality of the decision. An experienced executive will regard a readily achieved consensus within the policy-making group as a reason for *postponing* rather than taking action. Alfred P. Sloan, former chairman of General Motors, is reported to have said at a meeting of one of his top policy committees:

> "Gentlemen, I take it we are all in complete agreement on the decision here. . . . Then I propose we postpone further discussion of this matter until our next meeting to give ourselves time to develop disagreement and perhaps gain some understanding of what the decision is all about."[62]

Deferring action is more difficult, of course, when the situation seems to require it or when the chief executive himself is disposed to take immediate action. In certain types of international crisis a kind of spontaneous consensus may quickly emerge among members of the policy-making group on behalf of the "need for action" to prevent damage to U.S. interests—a consensus which may prevent adequate consideration of the magnitude of the expected damage and how much cost and risk one should undertake in order to prevent it.[63] It is particularly when everyone seems to agree on the need for some action to prevent expected damage that the most dangerous mistakes in the calculation of risk and utility are likely to be committed. The typical error under these circumstances is a gross underestimation of the costs and risks of the action taken. Conversely, *disagreement* within the decision-making group on the proper objectives, the proper means, the kinds and level of risk present in the situation, is more likely to improve the analytical process and the advice that precedes the final choice of policy the President makes.

This type of malfunction of the policy-making process can be vividly seen in the events leading to President Johnson's decision to send U.S. military forces into the Dominican Republic in the spring of 1965. As Philip Geyelin (a reporter for *The Wall Street Journal* at the time) put it in his excellent review of this crisis: "If Lyndon Johnson had acted much differently than he did in the early, decisive days of the Dominican crisis, he would have had to invent his own alternatives and ignore the counsel of his principal advisers. . . ."[64] There is nothing in the available record to indicate that the Special Assistant for National Security Affairs, McGeorge Bundy, attempted to preserve the President's options in this case.[65]

Indeed, all available accounts of the crisis indicate that consensus on the need for U.S. military intervention developed quickly, easily, and without challenge within the decision-making group. Geyelin notes that Johnson "was remarkably at the mercy of the advice and activities of his subordinates on the scene"—who speedily concluded that a rebel victory carried with it the risk of an eventual communist regime. This definition of the situation emerged almost at once in the field and was immediately accepted without question in Washington because everyone's perception of the crisis in the Dominican Republic was shaped by a strong policy predisposition that antedated the crisis. This was the belief that the Dominican Republic should not be allowed to become another Cuba. A distorted, exaggerated perception of the threat in Santo Domingo emerged when available information on the rebels was viewed through the prism of the "Cuban syndrome." No attempt was made until well after the U.S. was committed to intervention to check the U.S. Embassy's definition of the situation and of the policies most appropriate for meeting it.[66]

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[62] Quoted by Peter F. Drucker, *The Effective Executive* (N.Y.: Harper & Row, 1966), p. 148.

[63] Indeed, as in psychological experiments on decision making in crisis situations, fewer alternatives for dealing with the expected damage are likely to be identified and evaluated under these circumstances. Recent research of this kind is reported by James A. Robinson, Charles F. Hermann, and Margaret G. Hermann, "Search Under Crisis in Political Gaming and Simulation," in *Theory and Research on the Causes of War*, ed. Dean G. Pruitt and Richard C. Snyder (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969), pp. 80–94. The authors report results of simulation experiments that indicate that "under crisis, both the search for alternative courses of action and the actual number of such alternatives considered by the political decision-makers are reduced" (p. 91). They recognize, however, that earlier experiments have shown that "moderate stress produces creative decision-making, including search, and induces more search and innovation than either absence of stress or presence of intensive stress." (On this point see also Wilensky, *Organizational Intelligence*, p. 76ff. and Holsti, *Crisis, Escalation, War*, Chapter 1.) In fact, as in the Cuban missile crisis, when decision makers are alert to the possibility that a crisis atmosphere may disrupt rational decision making they may be able to design or introduce ad hoc procedures to safeguard against these tendencies.

[64] Philip Geyelin, *Lyndon B. Johnson and the World* (New York: Praeger, 1966), pp. 244–5.

[65] According to one source, which is very critical of McGeorge Bundy generally, Bundy favored U.S. intervention in the Dominican crisis. If so, it must have made it difficult to combine the role of policy adviser with that of "protecting the President's options" (David Halberstam, "The Expensive Education of McGeorge Bundy," *Harper's* [July, 1969], p. 23).

[66] Some of the major accounts on the Dominican intervention, in addition to Geyelin's book, are; John

But decisional premises of this kind do not always paralyze the ability of decision-makers to make a reasoned calculation of the utility of intervention. Vigorous multiple advocacy within the Eisenhower administration in the Indochina crisis of 1954 helped to control the effect that ideologically reinforced decisional premises were allowed to have on the final decision. The policy-making process worked much better in the 1954 crisis than in the Dominican crisis. In the spring of 1954 the Eisenhower Administration was suddenly faced with having to decide whether to intervene in Indochina. Intervention on behalf of the beleaguered French forces at Dienbienphu was strongly favored by Chairman of the Joint Chiefs of Staff Admiral Radford and apparently also by Secretary of State John Foster Dulles,[67] and it was supported by other members of the administration. The psychological momentum for intervention took hold quickly. Advocates of intervention within the administration attempted to develop a wider consensus by emphasizing the expected damage to U.S. interests if Indochina were lost. These efforts were crowned by Eisenhower's invocation of the "row-of-dominoes" analogy.

The President was ambivalent about intervention, however, and had second thoughts about its costs and risks. He was unwilling in any case to act without consultation with congressional leaders. Some observers feel that the President deliberately slowed up the momentum for intervention within his administration and indirectly strengthened the hand of those advisers who were opposed to it by directing Dulles and Radford to take their case to congressional leaders. In Congress it soon became apparent that the air strike advocated by Radford to relieve pressure on French forces at Dienbienphu did not have the backing of all members of the J.C.S. Moreover, it seemed highly probable that the air strike would fail to achieve its purpose and become instead the first step on an exceedingly slippery slope that could lead the United States into another major ground war on the Asian mainland.

As a result, a wholesome sobriety was introduced into policy calculations. The momentum for intervention was slowed down and eventually reversed, even *despite* the fact that the Cold War image of communism and a perception of threatened damage to U.S. interests were widely shared by policy makers and congressional leaders. Given the strength of this decisional premise and the initial definition of the situation, intervention could be avoided only because the *process* of policy making worked in such a way as to force decision makers to face up to the sobering question of the costs and risks of intervention. With the memory of the recent Korean War still fresh, some policy advisers insisted that the administration face up realistically to the level of effort that would be required to win in Indochina before taking the first step.

General Matthew Ridgway, then Chief of Staff of the Army, played the leading role in this respect. At the first indication that advocates of intervention were considering the use of air and naval forces in Indochina, Ridgway labeled the proposed action "ominous." He warned that a commitment of air and naval forces would inevitably lead to use of ground forces. It was "incredible" to him that Washington should forget the bitter lesson of Korea so soon. Determined that the administration should face up to the costs of intervention, Ridgway quickly dispatched a group of army specialists under General Gavin to Indochina to determine at first hand the difficulties and requirements of a U.S. intervention. On the basis of this study, Ridgway reported to Eisenhower that the cost in men and money of successful intervention in Indochina would be greater than in Korea.

While it is difficult to ascertain the precise impact of Ridgway's advice on Eisenhower's decision, Ridgway himself believed that his report, brought before the National Security Council, played a considerable, perhaps decisive role in persuading the administration

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Bartlow Martin, *Overtaken by Events* (New York: Doubleday, 1966); Theodore Draper, *The Dominican Revolt* (N.Y.: Commentary, 1968); Tad Szulc, *Dominican Diary* (N.Y.: Delacorte Press, 1965); Rowland Evans and Robert Novak, *Lyndon B. Johnson: The Exercise of Power* (N.Y.: The New American Library, 1966); Dan Kurzman, *Santa Domingo: Revolt of the Damned* (N.Y.: Putnam, 1965); Center for Strategic Studies, Georgetown University, *Dominican Action—1965: Intervention or Cooperation?* (Washington, D.C.: Georgetown University Press, 1966); Haynes Johnson and Bernard M. Gwertzman, *Fulbright: The Dissenter* (N.Y.: Doubleday, 1968); Jerome Slater, *Intervention and Negotiation* (N.Y.: Harper & Row, 1970); in addition, I have drawn on two seminar papers on the Dominican intervention by Stanford University graduate students, Robert Service and Robert G. Weinland.

[67] In a recent detailed analysis of available sources, Robert F. Randle concedes that Dulles's reaction to Radford's initial proposal for U.S. air strikes to relieve pressure on Dienbienphu is not known, but offers the interpretation that Dulles did not really favor the plan (*Geneva 1954* (Princeton: Princeton University Press, 1969), pp. 65, 97). Randle's interpretation of Dulles's attitude contradicts that of other scholars; but the question of Dulles's position on the matter is not critical for the analysis in this paper.

against intervention.[68]

2. *When advisers and advocates take different positions and debate them before the President but their disagreements do not cover the full range of relevant hypotheses and alternative options.*

There is no assurance that on any given policy problem each of the relevant options will find an advocate within the policy-making group. Some options may fail to get serious consideration because it is felt that the President has excluded them or would reject them, or because they are not in the interest of any agency and no one wants to pay the bureaucratic cost of asking the President to adopt them. Thus, the option of not giving direct military assistance to South Korea when it was attacked by North Korea in June 1950 was never really considered by Truman's advisers, in part because the President had made it clear at the outset that the United States would not allow the attack to succeed. (A fuller account of this case is given below.)

Other options may fail to get consideration because policy-making officials serving under the chief executive are disinterested in them. There are various ways of reducing the executive's freedom of action even while seemingly providing him with multiple options. As Robert H. Johnson notes: "One of the chief problems with attempts to lay out major policy alternatives is the strong temptation to load the dice. A typical procedure is to set up a straw-man alternative on either side of a middle course of action which quickly becomes the logical choice over the more 'extremist' options."[69] Whether or not this type of malfunction took place during the planning of the Vietnam escalation is uncertain.[70] Possibly it did occur to some extent and for a combination of the two reasons indicated. President Johnson's advisers may have felt that he had already excluded the withdrawal option and, besides, many of them were against withdrawal and were not motivated therefore to provide a well-considered, persuasive version of this option. The Pentagon Papers give the impression that most of the principals in the policy-making system had for some time come to feel that the United States could not withdraw from South Vietnam but would have to engage in some degree of military escalation, if necessary. During 1964 President Johnson himself was largely preoccupied with the forthcoming presidential election and with domestic policy; he did not participate actively in the extensive planning activities that went on during the first ten months of the year. While Johnson apparently indicated that he wanted all options considered, there is no indication that the option of U.S. withdrawal became the subject of major study before the election.[71]

After Johnson's election in early November, his advisers succeeded in drawing him into serious discussion of the situation in Vietnam and consideration of additional U.S. assistance. The internal political situation in Vietnam was now so unstable, however, that in the policy review of late November it was agreed that no increase in assistance by the United States was to be contemplated until a more stable government and political climate emerged in Saigon. Given little attention in the deliberations of American policy makers was the sound (and not unfamiliar) proposition that the United States should not lend military and economic aid to regimes which lack the will to help themselves, the capacity to govern, and sufficient popular support. In the important policy review of late November which, as it turned out, afforded the last opportunity for calm, unhurried deliberation on the situation, there was little disposition to consider whether or not the U.S. should now face up to the necessity or desirability of withdrawing from South Vietnam on the grounds that the prerequisites for continued, let alone increased, U.S. involvement were totally lacking.

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[68] Ridgway's claim, however, cannot be easily verified on the basis of existing sources, which do not pinpoint the date of its completion and submission closely enough to judge at what point it entered into policy making. On the other hand, knowledge of Ridgway's strong opposition to intervention and of the fact that he was preparing a detailed study to back his position may have had some impact even though, as seems to be the case, the report was formally submitted in late April after Eisenhower made his decision not to intervene. More generally, as Stanford University student Stuart Baskin emphasizes in a recent seminar paper (December, 1971), the level of information and analysis on the situation throughout the government was remarkably poor both before and during the crisis, with the belated exception of Ridgway's report. It must be said, therefore, that while multiple advocacy was unusually vigorous and effective in this case, none of the participants in the policy debate possessed an adequate analytical basis for his statements.

See also Matthew B. Ridgway, *Soldier* (N.Y.: Harper & Row, 1956); also "What Ridgway Told Ike—War in Indochina Would be Tougher than Korea," *U.S. News & World Report*, June 25, 1954; Maxwell D. Taylor, *The Uncertain Trumpet* (N.Y.: Harper & Row, 1959), p. 24; Melvin Gurtov, *The First Vietnam Crisis* (N.Y.: Columbia University Press, 1967), pp. 125–6, 136, 146; *The Pentagon Papers* (N.Y.: N.Y. Times, Bantam Books, 1971), Ch. 1; *The Senator Gravel Edition: The Pentagon Papers* (Beacon Press, 1971), Vol. I. (I am also indebted to Jane Howland and Peter Fitzpatrick for earlier research on this case, in which many other sources were also consulted.)

[69] "National Security Council," p. 723. Others who have called attention to the subterfuge of "straw man" options include Destler, *Presidents, Bureaucrats, and Foreign Policy*, Ch. 5; Leacocos, "Kissinger's Apparat," p. 24; Charles Yost, "On Affairs at State," *N.Y. Times*, May 30, 1971.

[70] Of the many accounts and sources available in addition to *The Pentagon Papers*, see particularly James C. Thomson, Jr., "How Could Vietnam Happen?" (*Atlantic Monthly*, 1968); Philip Geyelin, *Lyndon B. Johnson and the World* (N.Y.: Praeger, 1966); Richard M. Pfeffer, ed., *No More Vietnams?* (N.Y.: Harper & Row, 1968); Townsend Hoopes, *The Limits of Intervention* (N.Y.: David McKay, 1969); Chester Cooper, *The Lost Crusade: America in Vietnam* (N.Y.: Dodd, Mead & Co., 1970); Marvin Kalb and Elie Abel, *Roots of Involvement* (N.Y.: Norton, 1971); Lyndon B. Johnson, *The Vantage Point* (N.Y.: Holt, Rinehart and Winston, 1971), Ch. 6.

[71] Thomson, "How Could Vietnam Happen?" (p. 52) goes so far as to assert that Johnson's advisers converged on the option of bombing North Vietnam despite his request for all the options. On the other hand, other sources suggest that on more than one occasion Johnson gave his advisers clear indications that he did not intend to lose South Vietnam.

Noteworthy is the attenuated, ineffectual role of a heretofore important decisional premise. Ever since the Chinese civil war, American policy towards Asian countries subjected to the threat of communism had been constrained by the proposition that the United States should not become inextricably committed via economic and military aid to regimes which lacked the will to help themselves, the capacity to govern, and sufficient popular support. The logical implication of this decisional premise, given the internal political situation in South Vietnam, was that further involvement should be avoided and that, instead, preparations be made for a face-saving exit from South Vietnam. No one in the top-level group of policy makers attempted to make a case for this option, however; and neither the President nor, apparently, his Special Assistant for National Security Affairs, McGeorge Bundy, requested that this option, too, should be given careful consideration.

To be sure, elements of this point of view were clearly articulated in November by George Ball, Undersecretary of State, who emerged as the focal point of a weak "dovish" coalition at lower levels in the policy-making system. But Ball's viewpoint was late in emerging and indeed it never entered the mainstream of policy planning on Vietnam. He was "heard" in November, but not "listened to" in part because he was regarded as a "Europe man" who did not share the top policy makers' premise that Southeast Asia had become strategically important to the United States.[72] The importance of this new decisional premise cannot be minimized. The Pentagon Papers indicate that it had gradually emerged in the preceding years and had come to be accepted by most, if not all, of the principal policy makers. Certainly it succeeded in neutralizing the earlier decisional premise to which we have referred, evidently without the conflict between the two premises being clearly identified and subjected to a major policy debate. In the end, this new decisional premise restricted the scope of multiple advocacy; it dominated policy planning and the decision for escalation.[73]

3. *When there is no advocate for an unpopular policy option.*

In the Vietnam escalation case we have just reviewed there was, at least, an advocate for the unpopular option of withdrawal in the person of George Ball. In the Korean case, by contrast, the policy-making group assembled by President Truman on the first day of the North Korean invasion quickly developed a consensus on the need for action to prevent the expected damage to U.S. interests. While members of the group were by no means uniformly enthusiastic at the prospect of U.S. military involvement, the consensus to act was at no time subjected to challenge. This was not because of any previously agreed-upon contingency plans which had only to be put into operation. Indeed no such plans existed. The North Korean attack came as an unexpected shock. It was this rude reality, not the availability of pre-existing plans, that helped produce consensus within the policy-making group. Critical in this respect, too, was the President's initial definition of the situation as one in which too much was at stake to permit the United States to acquiesce.[74] The ingredients for bureaucratic politics and multiple advocacy were present in the long-standing policy and personal conflicts between Secretary of State Dean Acheson and Secretary of Defense Louis Johnson, but their expression was muted by the President's attitude, the atmosphere of crisis, and Acheson's reversal of Administration policy on Formosa in the direction that Johnson had advocated.

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[72] Ball avoids discussion of his devil's advocate role in his book, *The Discipline of Power* (Boston: Little, Brown & Co., 1968). His participation in the policy debate in November, 1964, is documented in the Pentagon Papers and he has referred to it publicly on a number of occasions.

Vice-President Humphrey is reported to have made a last-ditch attempt, following the VC attack on Pleiku in February, 1965, to prevent the bombing of North Vietnam. According to Townsend Hoopes, former undersecretary of the Air Force, Humphrey's views were received at the White House "with particular coldness, and he was banished from the inner councils for some months thereafter, until he decided to 'get back on the team'" (Hoopes, *Limits of Intervention*, p. 31).

[73] There were, of course, other important decisional premises as well. Leslie H. Gelb attaches importance, among other things, to the belief that loss of South Vietnam would have a variety of severe domestic political consequences ("Vietnam: How the System Worked," *Foreign Policy* [Summer, 1971], 140–167). Daniel Ellsberg places central emphasis on Gelb's point in his own interpretation, "The Quagmire Myth and the Stalemate Machine," *Public Policy*, 19 (Spring 1971) 217–274.

As a result, the military pressures of the quickly deteriorating situation in Korea determined the choices made. The participants in these decisions believed they had few alternatives to the actions they took. Indeed, in one sense of the word no "decision" about intervention was made; that is, the President and his advisers did not at any point sit down to weigh the political and military factors for and against direct U.S. military intervention. There was no search for options, for alternative ways of limiting the expected damage to U.S. national interests. Decisional premises and assumptions that shaped the definition of the situation in such a way as to make U.S. intervention a necessary response—such as the belief that the aggression in Korea was like Nazi aggression in the 'thirties and, if unopposed, would also encourage communists to undertake new aggressions—were not singled out for any kind of critical examination.

During the first few days of the conflict, at a time when U.S. assistance was still limited to air and naval support, several of Truman's advisers expressed their reluctance to see U.S. ground troops committed to combat in South Korea. Indeed, the President's own hesitation in the matter was also noticeable. There was some discussion of the disadvantages and hazards of this course of action before the need for U.S. combat troops became evident. Once the deteriorating battlefield situation made it imperative, however, no adviser questioned the introduction of American combat forces except George Kennan, who hesitated on the grounds that it would increase the likelihood of Soviet counterintervention. Neither Kennan nor military advisers who were unenthusiastic about being drawn into a land war on the Asian continent were given any encouragement to play a more vigorous advocate's role in the policy discussions.[75]

Nor was an effort made to calculate soberly the level of military effort that would be required and the full costs of attempting to defend South Korea. General MacArthur's recommendation of introducing U.S. ground combat forces was accepted unquestioningly on the basis of his quickly improvised estimate that two divisions under his command from Japan would enable "early offensive action" to save South Korea. The seemingly high value that policy makers attached to the objective of resisting the North Korean aggression was not tested by calculating realistically what the price tag might turn out to be.

Later in the Korean War, when the decision was being made to send U.S. and U.N. forces across the 38th parallel to pursue the defeated North Korean army and to unite the two parts of the country, the threat of Chinese Communist intervention became a factor. Important officials in Truman's circle of policy advisers (Secretary of the Air Force Thomas Finletter and Chief of Naval Operations Admiral Forrest Sherman, as well as George Kennan) were disturbed by the risks of Chinese intervention.[76] Once again, however, Truman's structuring and management of the policy-making process discouraged those with reservations about the drift of policy from playing the role of more articulate devil's advocates. Indeed, such a role was not generally congenial to President Truman. He preferred to structure the policy-forming process in terms of functional expertise. Each agency head was expected to provide his considered view of that aspect of the problem for which he was responsible. Within each department second- and third-level officials contributed their advice to their department head, and deviations from this hierarchically organized flow of advice within each department were discouraged. Kennan's advice on this and other occasions when it differed from Acheson's was overshadowed and controlled by the advice of the Secretary of State.[77]

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[74] Taking note of this, Richard C. Snyder and Glenn D. Paige emphasize that the definition of a situation itself can, in effect, exclude consideration of any but a single course of action; see "The United States Decision to Resist Aggression in Korea," *Administrative Science Quarterly*, 3 (December, 1958) 245; Harry S. Truman, *Memoirs: Years of Trial and Hope*, Vol. II, Garden City: Doubleday & Co., 1956, pp. 332ff; Glenn D. Paige, *The Korean Decision*, pp. 98–99, 113–115, 124–6, 148–9, 174; Alexander L. George, "American Policy-making and the North Korean Aggression," *World Politics*, 7 (Jan., 1955), 209–32.

[75] Glenn D. Paige, *The Korean Decision*, N.Y.: The Free Press, 1968; p. 136, 164–5, 255, 257, 260–1, 300–1. Kennan's hesitation over use of U.S. ground troops, however, should not be exaggerated into making him, as some revisionist writers have tried to do, into an opponent of U.S. intervention. In his memoirs Kennan states that from the onset of the Korean crisis he felt that the United States would have to react with all necessary force to repel the attack and to expel North Korean forces from South Korea. He also favored prompt steps to protect Formosa from falling into Communist hands on the ground that "two such reverses coming one on the heel of the other could easily prove disastrous to our prestige and to our entire position in the Far East" (*Memoirs, 1925–1950*, [Boston: Little, Brown & Co., 1967], p. 486).

[76] Kennan, pp. 487–96; also Trumbull Higgins, *Korea and the Fall of MacArthur* (N.Y.: Oxford University Press, 1960), pp. 78–79; Robert E. Osgood, *Limited War* (Chicago: University of Chicago Press, 1957), pp. 183–84.

4. *When advisers to the President thrash out their own disagreements over policy without the President's knowledge and confront him with a unanimous recommendation.*

In this variant of the workings of bureaucratic politics the other actors in effect "gang up" on the chief executive and try to sell him the policy they have worked out among themselves. Clark Clifford gives a succinct, authoritative account of this practice in an interview describing his role as assistant to President Truman on domestic affairs:

> The idea was that the six or eight of us would try to come to an understanding among ourselves on what directions we would like the President to take on any given issue. And then, quietly and unobtrusively, each in his own way, we would try to steer the President in that direction. . . . Well, it was two forces fighting for the mind of the President, that's really what it was. It was completely unpublicized, and I don't think Mr. Truman ever realized it was going on. . . .[78]

In this case, as Clifford's account makes clear, two coalitions of advisers were competing for influence over policy. A more dangerous situation arises when all major advisers on a policy issue reach an agreement among themselves before going to the executive. This type of malfunction of the process almost occurred in the late autumn of 1964 when President Johnson was confronted with a solid line-up of advisers recommending that he proceed with the Multilateral Force for NATO. The M.L.F. was a strategic force to be composed of surface ships manned by mixed crews drawn from a number of NATO countries. For several years a small but strong group of M.L.F. partisans within the administration had pushed this idea as a way of knitting the alliance together. An opportunity for final U.S. approval of the plan and its implementation arose in connection with Prime Minister Harold Wilson's visit to Washington for discussions with the President. The advocates of the M.L.F. within the administration succeeded in coordinating with all of the President's chief advisers a position paper that would finally have committed the U.S. firmly to the M.L.F. Hence, on the eve of his critical meeting with the British Prime Minister, Johnson "was confronted with a spirited, consecrated, nearly united bureaucracy. . . ."[79]

Now, it is extremely difficult for a President to act contrary to the unanimous advice of all his national security advisers. It is well to regard this variant of the bureaucratic politics process as a possible malfunction, for its occurrence threatens to deprive the chief executive of an adequate evaluation of available options. The President does not benefit adequately from multiple advocacy when the actors thrash out or compromise their differences *privately* and confront him with a unified recommendation. It is for this reason one of the conditions for effective multiple advocacy stressed earlier is presidential-level participation in or, at least, sensitivity to the day-to-day workings of the bureaucratic political aspects of policy making. Since the chief executive himself cannot be expected to participate directly in this process on every occasion or even to monitor it closely, clearly this participation becomes a critical task for his personal assistants. It is only through alter egos who clearly understand the President's responsibilities and needs in this respect that the chief executive can monitor the workings of the policy-forming system and intervene as necessary to correct malfunctions of process before they have an adverse effect on his policy choices.

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[77] In a laudatory account of the way in which Truman organized and managed the process of foreign policy making, Dean Acheson credits him with infusing an adversary process into N.S.C. meetings similar to that of the law court. It is clear that what Acheson endorses in this respect is the formal, orderly variant of the adversary process—one which worked to Acheson's advantage because Truman acknowledged and leaned on his special competence in foreign affairs—and not the unstructured and unpredictable variants of multiple advocacy associated with the game of bureaucratic politics. Acheson's distaste for unstructured, informal variants of multiple advocacy—in which he enjoyed less influence—emerges clearly from his critical account of the workings of the ad hoc Executive Committee of the N.S.C. at the time of the Cuban missile crisis, in which Acheson participated at President Kennedy's invitation (Acheson, *Present at the Creation,* [N.Y.: Norton, 1969], pp. 733–737; see also Acheson's review of R. F. Kennedy's *Thirteen Days* in *Esquire,* February 1969).

[78] Patrick Anderson, *The Presidents' Men* (Garden City, N.Y.: Doubleday, 1968), p. 116.

[79] Geyelin, *LBJ and the World,* p. 162. A detailed analysis of the workings of the bureaucratic politics process leading up to the final confrontation of the M.L.F. advocates with President Johnson, based partly on interviews, is provided in John D. Steinbruner, "The Mind and Milieu of Policy-makers: A Case Study of the M.L.F." Ph.D. dissertation, Department of Political Science, M.I.T., February 1968. For this portion of the case study, however, Steinbruner relies on Geyelin's account, which he regards as accurate.

As a matter of fact, this critical task was performed ably by one of the President's alter egos in the M.L.F. case just reviewed. Geyelin states that the position of McGeorge Bundy, Special Assistant for National Security Affairs, as "guardian of options and protector of the President was perhaps never more effectively displayed than in the episode of the M.L.F. Bundy had sensed trouble building up earlier in the year. With the M.L.F. partisans in full cry, the President's position was uncertain. . . ."[80] Sensing the development of the type of malfunction we have been discussing, Bundy initiated a strategy of preventive intervention to restore some semblance of multiple advocacy to the system. He quietly called upon Richard Neustadt, a part-time consultant to the White House, to make an independent appraisal, for the President's benefit, of the M.L.F. issue in Europe as well as in Washington. Armed with this and other information which Bundy assembled, the President entered the final briefing conferences with his foreign policy advisers prepared to challenge the decisional premises, information, and recommendation of their position paper on the M.L.F. "In the course of the protracted conferences in preparation for the Wilson visit," Geyelin reports, "Johnson assailed the men around him, questioning their competence as well as their counsel. . . ."[81]

In emphasizing the importance of Bundy's intervention in this case, we do not ignore that other factors also worked against Johnson's adherence to the M.L.F. There was by no means a clear consensus on its behalf, let alone enthusiasm, within NATO; and senior members of Congress as well as many officials in the executive branch were not enthusiastic supporters of the M.L.F. What Bundy's timely intervention accomplished was to bring these factors into greater prominence for the President's benefit.

5. *When advisers agree privately among themselves that the President should face up to a difficult decision, but no one is willing to alert him to the need for doing so.*

In direct contrast to the malfunction just discussed, in which his advisers privately agree upon a policy which they then attempt to get the President to adopt, this malfunction refers to the unusual and dangerous situation in which a private consensus on the need for presidential action emerges among his advisers but is not communicated to the President. This highlights once again the critical importance of presidential-level participation and sensitivity to policy-making discussions within the system.

In the M.L.F. case, we have seen, monitoring of these discussions by the Special Assistant for National Security Affairs alerted the President in time. But in a different crisis under Truman in November, 1950, failure to alert the President resulted in a classic example of this kind of breakdown in the advisory system. By early November, large numbers of Chinese Communist forces had already intervened in the Korean War and had subjected U.S. and South Korean forces to sharp tactical combat; but they had not yet launched an all-out offensive against U.S. and U.N. forces. Nonetheless, Truman's chief civilian and military advisers in Washington were acutely concerned over the risks associated with the maldeployment of MacArthur's forces in North Korea in the presence of large numbers of Chinese Communist forces. The President's advisers seemingly agreed among themselves that MacArthur's directives should be changed in order to reduce the vulnerability of his forces, but this consensus was not translated into action. According to Richard Neustadt's account, each adviser had his own reasons for not taking the problem to Truman. And each adviser interpreted his official role quite narrowly in order to relieve himself of the obligation to take the initiative.

"No one went to Truman," Neustadt writes, "because everyone thought someone else should go." He continues:

> The military chiefs deferred to State; let Acheson, as guardian of 'policy', ask Truman to reverse MacArthur. But Acheson, already under fire from the Capitol, was treading warily between the Pentagon and that inveterate idealist about generals, Harry Truman. In immediate terms the risk was 'military'; if it justified reversing the commander in the field, then the joint Chiefs must make the judgment and tell Truman. So Acheson is said to have insisted, understandably enough, and there the matter rested.

As for Secretary of Defense George Marshall, who had preceded Acheson at State and had himself been Army Chief of Staff when Bradley (now Chairman of the Joint Chiefs of Staff) was subordinate commander, he had "leaned over backwards" since returning to the government as Secretary of Defense shortly before these events took place

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[80] Geyelin, p. 170. Other accounts represent McGeorge Bundy intervening not as a neutral watchdog of the President's options but as a concealed advocate against the M.L.F. who had bided his time. (Patrick Anderson, *The Presidents' Men*, pp. 271–2; David Halberstam, "The Expensive Education of McGeorge Bundy," *Harper's*, July 1969, p. 29.)

[81] Geyelin, p. 162.

> not to meddle with the work of his successors in *their* jobs. He had also leaned over backwards not to revive the old Army feud between him and MacArthur. What Acheson and Bradley were not ready to initiate, Marshall evidently felt he could not take upon himself. . . . The President, meanwhile, had little thought of over-riding, on his own, the tactical decisions of a qualified commander.[82]

This was a sorry example, indeed, of narrow bureaucratic role-playing at the highest advisory level. One can only speculate what Truman's response would have been had his advisers shared their concern with him and recommended that MacArthur's directives be changed. If Truman had acted promptly, there would have been time to pull back MacArthur's forces before the Chinese launched their major offensive on November 28th. The catastrophe that followed might have been avoided altogether or greatly reduced.[83]

6. *When the President, faced with an important problem to decide, is dependent upon a single channel of information.*

A seldom-noted aspect of Khrushchev's behavior during the Cuban missile crisis was that he quickly established multiple channels for securing information on Kennedy's intentions. Too much was at stake for the Soviet government for it to wait passively for Washington to provide deliberate or inadvertent signals regarding the President's intentions. Instead, the Soviet government initiated attempts of its own through several channels to secure such information—for example at the U.N.; through the Soviet Ambassador's conversations with Robert Kennedy and other members of the Administration; through the special contacts that Alexander Fomin, the top intelligence specialist in the Soviet Embassy in Washington, established with John Scali, an American journalist known to have high-level contacts in the State Department.

Faced with the need to make important decisions momentarily, the Soviet premier grasped the value of redundancy in information coverage of critical aspects of his opponent's behavior.[84] In striking contrast, U.S. leaders have allowed themselves in several crises to remain dependent on a single channel of information about critical aspects of the situation. Among the many malfunctions of the policy-making process evident in planning the Bay of Pigs fiasco in 1961 was the fact that Kennedy and his advisers, including the JCS, depended on CIA's estimates of Castro's military and political strength. Both were miscalculated and underestimated by CIA. It was incorrectly estimated that there was a substantial anti-Castro underground, which would lead to an uprising when the invasion by Cuban exiles took place. It was erroneously believed that Castro's air force was weak and vulnerable and that he did not have the air power to defeat the invading force. CIA argued that the invasion should not be delayed since Cuba would soon receive modern air power from the Soviets. As a matter of fact Castro had already received these modern aircraft.[85]

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[82] Richard Neustadt, *Presidential Power* (N.Y.: Wiley, 1960), p. 145. Neustadt makes use in his account of Martin Lichterman's correspondence and an interview with Acheson, reported in Lichterman, "To the Yalu and Back," in *American Civil-Military Decisions,* ed. Harold Stein (University of Alabama Press, 1963), p. 602.

[83] Neustadt's and Lichterman's accounts of these events were qualified some years later both by Dean Acheson and General J. Lawton Collins, Army Chief of Staff at the time. (Acheson, *Present at the Creation,* pp. 466–68, 754; Collins, *War in Peacetime: The History and Lessons of Korea* [Boston: Houghton Mifflin Co., 1969], pp. 202, 205–217.) Neustadt's interpretation was questioned earlier also by David S. McLellan in his "Dean Acheson and the Korean War," *Political Science Quarterly,* 83 (March 1968), 16–39.

McLellan agrees that Acheson and other top-level advisers did labor under the "dread of Chinese involvement" in *early* November. He notes, however, that at two important policy meetings on November 21, 1950, Acheson displayed relative optimism that MacArthur could accomplish his mission. McLellan is perplexed by this marked change of mood. He fails to see that it can be traced to the Chinese disengagement on the battlefield that took place after the high-point of Washington's (and MacArthur's) anxiety on November 9. When day after day passed without further combat or major contact with Chinese forces, the acute anxiety American leaders had felt in early November gradually declined. It is evident from McLellan's account of the November 21 policy meetings—the content of which is disclosed for the first time by McLellan—that Acheson had now fully overcome the concern he and others had felt earlier in the month about the riskiness of the military strategy MacArthur was bent on pursuing. The seeming disappearance of the Chinese forces and the continued quiescence of the battlefield evidently strengthened hopes in Washington that MacArthur might succeed after all in repelling Chinese intervention. Having considered the need for a change in presidential directives to MacArthur earlier in November without acting to bring it about, Acheson at a meeting with his staff on November 21 expressed the belief that the directive to MacArthur should not be changed until the General had had a chance to probe the situation and push forward his planned offensive. The fact of this change of mood in Washington was carefully withheld by administration witnesses during the Congressional hearings on MacArthur's dismissal in 1951. Such a disclosure would have been damaging to the administration, which represented itself as having remained deeply concerned about MacArthur's military operations up to the point at which the Chinese launched their all-out offensive in late November.

[84] On this general point see also O. R. Holsti, *Crisis, Escalation, War,* p. 21.

Although the single channel of information on these and other inputs to policy planning was controlled by CIA, where the chief advocates and planners of the invasion resided, Kennedy's suspicions were not aroused. There was ample time to set up independent channels of information and intelligence evaluation, but the President and his alter egos did not move in this direction. Rather they allowed CIA to maintain unchallenged its position as dominant advocate, a position which rested partly on its exclusive custodianship of critical information and intelligence inputs.

The Bay of Pigs disaster made the White House reluctant to rely on established procedures existing in other departments and agencies for the selection and analysis of information. Shortly thereafter a "situation room" was set up in the White House basement adjoining the offices of the NSC staff. Teletype machines were installed to receive all important messages from military, diplomatic, and intelligence centers around the world simultaneously with their arrival at the State Department, the Department of Defense, and CIA. While the purpose of the "situation room" was ostensibly to improve the President's ability to manage crises and to stay on top of fast-breaking events, by equalizing the White House's access to critical information, it also improved its ability to deal with other actors in the bureaucratic politics system. Thereby the position of the Special Assistant for National Security Affairs was appreciably strengthened. "Immediate access to routine cables made it possible for Bundy and his staff to deal with the bureaucracy on an informed basis."[86]

Timely access to *available* information, however, does not in itself assure that multiple channels will exist when they are most needed. The type of malfunction we have been discussing occurred again in the Dominican intervention of April, 1965. For its picture of developments in the complex internal political situation in the Dominican Republic, Washington was dependent on a single channel of information—this time the U.S. Embassy in Santo Domingo. There was no independent source of information on these events.[87] Accordingly, had Johnson (or any of his advisers) wished to act differently in this crisis, as Geyelin puts it, "he would have had to discount the overwhelming weight of intelligence he received from the scene. . . ."[88]

Washington's dependence on a single channel of intelligence cannot be explained on the ground that the crisis developed too swiftly to initiate additional channels. Warning was available that the internal political conflict in the Dominican Republic might get out of hand. But neither the President nor his Special Assistant for National Security Affairs, "the guardian of presidential options," utilized the available warning to establish quickly the necessary additional independent sources of information on what was going on in the Dominican Republic.

7. *When the key assumptions and premises of a plan have been evaluated only by the advocates of that option.*

A striking example of this type of malfunction occurred in the Bay of Pigs case. Because Kennedy persistently entertained grave doubts about the CIA invasion plan and, moreover, did *not* regard Castro as a direct threat to the United States, it is puzzling that the key premises of the CIA plan were not subjected to thoroughgoing scrutiny. As Irving Janis emphasizes in his forthcoming study, *Victims of Groupthink,* the answer lies partly in the considerable respect and prestige enjoyed in the new Administration by CIA Chief Allen Dulles and Richard Bissell, both carry-overs from the Eisenhower administration, and partly also in Kennedy's wish to have them join his team.

Kennedy's reservations about the invasion plan were never translated into an effective search for an alternative. Instead, a number of partial constraints were imposed on the plan to make it more acceptable to the President.[89] Questions that the President and other policy makers raised from time to time were answered only by those who were preparing the plan and supporting it. As Irving Janis notes, the President let these meetings degenerate into question-and-answer periods. When an occasional skeptic, like Senator Fulbright, who was invited by the President to attend one of the policy meetings, raised questions about the plan, Kennedy was satisfied to allow the CIA representatives to respond. In the last analysis, the key assumptions and premises of the invasion plan were not subjected to thorough independent analysis because the President allowed the CIA to dominate and weaken the multiple advocacy system. The JCS, it is true, was asked to evaluate the CIA plan, but there was no disposition on the part of Kennedy or his leading advisers to ensure that the JCS was sufficiently motivated to give it a properly critical scrutiny, or to look closely at the qualified endorsement the JCS came up with.

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[85] Theodore C. Sorensen, *Kennedy* (N.Y.: Harper & Row, 1965), p. 302. Cf. also the detailed account of flaws in decision making in the Bay of Pigs case in Arthur Schlesinger, Jr., *A Thousand Days,* Chapter 10; and Chester Bowles, *Promises to Keep* (N.Y.: Harper & Row, 1971), p. 326.

[86] Anderson, *The Presidents' Men,* pp. 264–5. At the same time, the creation of the "situation room" paved the way for the NSC staff's extensive involvement in the internal activities of the departments and agencies. (K. C. Clark and Lawrence J. Legere, eds., *The President and the Management of National Security,* [N.Y.: Praeger, 1969], pp. 73–4.)

[87] A conscious attempt by Washington to develop an alternative source of information on events in the Dominican Republic was undertaken finally on April 29 *after* the U.S. decision to intervene overtly had been made and was being implemented. This was the Presidential mission of John Bartlow Martin.

[88] Geyelin, *LBJ and the World,* p. 244–5.

[89] Sorenson, pp. 304, 306; Schlesinger, ch. 10.

In providing consistently reassuring answers, the CIA representatives were not necessarily engaged in conscious deception, though wishful thinking may have been at work in their assessment of uncertainties. This reassurance is not surprising if we keep in mind that the CIA leaders firmly believed that action against Castro was necessary. They had created and trained a Cuban exile force to carry out the invasion; the preparations had already achieved a certain bureaucratic momentum by the time the new President established himself in office and turned some of his attention to reviewing the plan.

8. *When the President asks advisers for their opinions on a preferred course of action but does not request a qualified group to examine more carefully the negative judgment offered by one or more advisers.*

It is by no means certain, we have seen, that multiple advocacy will come into play on any given policy issue or function effectively to illuminate all sides of the issue. We have noted the danger that may arise when no actor in the system is willing to speak up for an unpopular option or to oppose the group's preferred course of action. A different kind of malfunction occurs when advisers who give counsel that runs against the grain suffer from inadequate resources for advocacy in the competitive marketplace of bureaucratic politics. Maldistribution among the actors of informational and analytical resources, of formal status, of informal prestige and reputation, or access to presidential confidence, and so on, can severely weaken the actual workings of multiple advocacy and give lopsided advantages to some actors within the bureaucratic politics system.

As a result, the President may "hear" the negative opinion of an adviser but not really "listen." Or he may be satisfied all too easily with the seemingly impressive rebuttal of a more powerful and prestigious advocate. This type of malfunction of the policy process was evident in the deliberations leading up to the Bay of Pigs fiasco. On various occasions the CIA plan to invade Cuba was strongly opposed by individual advisers—Chester Bowles, Arthur Schlesinger, Senator Fulbright. All accounts of Kennedy's management of the policy-making process in this case make clear that far from seeking opportunities to encourage vigorous multiple advocacy, he was reluctant to see it develop and hoped to satisfy his own doubts about the plan by procedures which did not so directly challenge its advocates and supporters.

Following the President's cue, neither did McGeorge Bundy, his alter ego for national security affairs in the White House, nor Robert Kennedy, nor Secretary of State Rusk, attempt to initiate or encourage an independent evaluation of the plan. When Hilsman suggested that his office do so, Rusk refused on the grounds of secrecy. And at one point, Arthur Schlesinger, who had written two memos opposing the plan, was taken aside by Robert Kennedy and told to "lay off." Those who opposed the invasion were "heard" but given no encouragement to develop the case against it or to form themselves into a group that would look into the issues more thoroughly.

What this case shows, therefore, is that the advice of policy dissidents which remains the mere opinion of the individuals concerned will not suffice to check the momentum of a dominant policy faction that is attempting to control the President's decision. In the Bay of Pigs case the futility of the dissidents' efforts cannot be explained on the grounds that the President enthusiastically supported the policy option in question. In fact, he disagreed with the major premise on which the CIA plan was based—namely, that Castro constituted a threat to vital U.S. interests—and had various reservations about the plan itself. Rather the explanation must be sought in part, as Kennedy recognized later, in his failure to manage the policy-shaping process more effectively.

It is less certain whether this type of malfunction also occurred in the policy discussions that led to the increase in the number of U.S. military advisers in South Vietnam in late 1961.[90] Doubts and hesitations over this course of action were shared by important policy advisers as well as by President Kennedy himself. *The Pentagon Papers* disclose that at one point in the policy debate Secretary of State Rusk himself warned the President that to send 8,000 additional military advisers to Vietnam would mean that the United States would be irretrievably committed. On another occasion Undersecretary of State George Ball argued against more military assistance to Saigon because he feared it might lead eventually to a deployment of as many as 300,000 troops.

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[90] Among the numerous sources consulted for this case the following were particularly useful: Schlesinger; Sorensen; Roger Hilsman, *To Move A Nation* (N.Y.: Doubleday & Co., 1967); Chester Bowles, *Promises to Keep;* John K. Galbraith and Anthony Lewis, "The World Through Galbraith's Eyes," A Tape Recorded Conversation, *N.Y. Times Magazine,* Dec. 18, 1966; John D. Stempel, "Policy/Decision-Making in the Dept. of State: Vietnamese Problem, 1961–1965," Ph.D. dissertation, Political Science Department, Uni-

It must also be noted that advisers who favored additional escalation at this time did not disguise their belief that this decision was linked to the more fundamental question of whether the United States should now make a commitment to preserve South Vietnam, and that additional escalation might well be required later on. Nevertheless, despite these efforts of advisers on both sides of the issue to structure the President's choice in a clear-cut fashion, Kennedy approved some of the recommendations for additional U.S. assistance without clarifying the fundamental question of the extent of the U.S. commitment.

Although the President was aware that the real problem in South Vietnam was Diem's lack of political strength, he felt he had to do something to bolster the resistance of the Saigon regime for global cold war reasons, to protect his attempt to neutralize Laos, and perhaps as a gesture to propitiate hawks in his administration. Kennedy did not feel himself obliged to make a clear-cut decision; he very definitely preferred to equivocate and buy time. He did not share fully the urgency of the doubts and hesitations expressed by some of his advisers, and was not receptive to their warnings that sending additional military advisers would harden the U.S. commitment. As a result, the option of U.S. withdrawal did not attract the kind of vigorous policy advocacy for which some sentiment existed within the advisory group. The lesser possibility of working toward a neutral South Vietnam was advanced by Chester Bowles. Although this position was vigorously debated, and although the President did not reject it out of hand, he regarded it at best as a fall-back position in case the South Vietnamese proved unable to cope.

Indeed, despite much sentiment to the contrary within the administration, it now began to view the deteriorating situation in South Vietnam as a military problem requiring military measures. This decisional premise was rejected by important State Department advisers, who held the crisis in South Vietnam was a political one and deplored the tendency to rely on increased military assistance. They lacked, however, a strong enough bureaucratic power base from which to press their advice particularly as Secretary of State Rusk, overcoming his earlier hesitation, began increasingly to support prosecution of the war by military means.

9. *When the President is impressed by the consensus among his advisers on behalf of a particular policy but fails to ascertain how firm the consensus is, how it was achieved, and whether it is justified.*

We spoke earlier of a malfunction (number 4) similar to this one in some respects, in which the other participants in bureaucratic politics thrash out their disagreements on a policy issue privately, without the President's knowledge, and then confront him with a unanimous recommendation. But the chief executive may also be the victim of what Irving Janis calls an "illusory" consensus among his advisers that reflects a rather different working of the policy-forming system. Thus, at the important April 4, 1961, meeting it appeared to President Kennedy that there was no longer any opposition to the Bay of Pigs plan. Evidently he did not realize that the way in which the policy-forming process had been managed had discouraged the emergence of opposition. As Schlesinger recalled it, "Our meetings were taking place in a curious atmosphere of assumed consensus." And, as Sorensen puts it, the advice offered Kennedy "was not so unanimous or so well considered as it seemed."[91]

What this suggests, then, is that a chief executive and his alter egos must not take the consensus among policy advisers at face value. Particularly when the consensus is agreeable to him, the President must force himself to test it in order to establish whether it is complete or obscures important differences and unresolved issues. Similarly, he should ascertain what the consensus is based on and how it was achieved. Is it a well-considered consensus in which all actors have done their homework properly and interacted with each other in a joint problem-solving exercise? Or is it a manufactured or synthetic consensus obtained through the dominance of one policy clique, or through bargain-

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versity of California, Berkeley, 1965; George W. Ball, *Discipline of Power*, pp. 333–4; James Reston, *The Artillery of the Press*, pp. 25–6; Daniel Ellsberg, "The Quagmire Myth and the Stalemate Machine," *Public Policy*, 19 (Spring, 1971), 217–274; Arthur Schlesinger, Jr., "Eyeless in Indochina," *New York Review of Books*, October 21, 1971; *The Pentagon Papers*. The research assistance of Jane Howland and David K. Hall on this case is gratefully acknowledged.

[91] Schlesinger, *A Thousand Days*, pp. 250, 255; Sorensen, p. 305.

ing among the actors that resulted in a compromise that papers over difficult problems and shirks the task of identifying and evaluating relevant options?

Implications for Strengthening the Policy-Making System

Awareness of these possible malfunctions should enable the custodian of the policy-making process to identify their emergence in a timely fashion. The kinds of intervention strategies needed to correct or compensate for any emergent malfunction are mostly self-evident and have been suggested in the preceding review of historical cases. It seems highly appropriate that responsibility for discharging this critical task at the highest level of policy-making in the executive branch should be assigned to the Office of the Special Assistant for National Security Affairs. (Staff assistants to senior officials in departments can be assigned a similar task as custodians of the process of policy formation in their own agencies.)

The Special Assistant will be able to perform the critical tasks associated with the custodian's role *only if he scrupulously refrains from becoming an advocate himself*. Both the advocate's role and the custodian's role are necessary, but effective operation of the policy-making system requires that these two roles be kept separate. To combine them in the person of one official will create the basis for a severe role conflict. When important policy issues are being decided it would be extraordinary if an individual who is actively involved himself as an advocate of a particular option could also discharge the critical tasks of custodian of the process, for to do so might well hamper his efficacy as an advocate. To say that the role conflict might be avoided if the custodian confined himself to being a *policy adviser* without engaging in advocacy is to ignore the ease with which a policy adviser is drawn into advocacy, as well as the danger of covert advocacy disguised as disinterested advice.

The role of custodian is critical for effective operation of the policy system. Performance of the role can only be eroded and eventually robbed of its integrity if its incumbent is encouraged by the chief executive to become a policy adviser, or otherwise succumbs to the temptation of becoming an advocate. In arguing that the chief executive should clearly define the role of custodian to exclude advocacy I do not ignore the President's equally important need for foreign policy advisers and even advocates on his personal staff. I am simply urging that the roles be kept separate and not be assigned to one person. Other persons with special competence on foreign policy matters can be appointed by the chief executive as his advisers on substantive issues, leaving the Special Assistant to focus exclusively on the procedural aspects of policy making.

It should be noted that both Truman and Eisenhower attempted to prevent leading staff personnel of the National Security Council from acting as policy advocates or advisers. (At the same time, both presidents appointed a number of other persons as personal advisers on foreign policy but, significantly, did not give them any responsibility for managing the NSC policy-making system.) Under Truman, the NSC staff was headed by an Executive Secretary whose duties were carefully circumscribed. When Eisenhower created the Office of the Special Assistant for National Security Affairs, he excluded from its duties the role of policy adviser. Robert Cutler, the first incumbent of the Office of Special Assistant, reports that his role was defined by Eisenhower as that of a non-advocate and a non-freewheeler. Only rarely, Cutler has written, did he undertake to suggest an independent position of his own inside or outside the NSC. Such forays "would sometimes bring down on my head an adverse storm." Interestingly, however, he adds: "But if debate was intensified on a germane issue, it was worth a knock on the head."[92]

Under Kennedy, however, a basic duality emerged in the role of Special Assistant for National Security Affairs. In the looser organization of the NSC that Kennedy introduced, the Special Assistant emerged as an ad hoc policy adviser to the President; he was no longer restricted to being the neutral manager of well defined NSC procedures. While we do not yet have a careful, informed analysis of McGeorge Bundy's effort and those of his successors (Walt Rostow and Henry Kissinger) to combine the task of custodian of the process with that of adviser, there are indications that these two roles cannot easily be combined in one position.[93]

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[92] Robert Cutler, *No Time for Rest* (Boston: Little, Brown & Co., 1966), pp. 315–316. A useful, up-to-date review of the development of the NSC and the variations in its importance, organization and performance is provided in Clark and Legere, *President and Management of National Security* (see especially Chapter 4 by Richard M. Moose and Chapter 10 by John Ponturo). Useful materials are also contained in Patrick Anderson, *The Presidents' Men;* Robert H. Johnson, "The National Security Council: The Relevance of its Past to its Future," *Orbis*, 13 (Fall, 1969), 709–735; Paul Y. Hammond, *Organizing for Defense;* Senator Henry M. Jackson, ed., *The National Security Council* (N.Y.: Praeger, 1965).

[93] Patrick Anderson emphasizes the basic duality that affected Bundy's performance as Special Assistant. He suggests that Bundy was aware of the latent role-con-

The same kind of role conflict is likely to emerge if the Special Assistant acts as a *spokesman* on behalf of existing policy. Whether he speaks openly to explain and support decisions taken by the administration or does so in an ostensibly off-the-record manner, this additional task may constrain his performance as neutral custodian of the process. Note that the definition of Robert Cutler's role in this respect restricted him to the classical anonymity expected of a presidential assistant. He was not to issue public statements, nor provide the press with rationales for policy decisions, nor even to discuss or write about how the NSC was organized and operated except as specifically authorized by the President.[94]

In the past decade there has been a gradual and cumulative change also in the way in which the "spokesman" component of the role has been defined. Beginning with McGeorge Bundy in 1961, the man serving as Special Assistant has increasingly acted as a major, though not the sole, spokesman for the President's foreign policy decisions. The explanation for this development is not difficult to grasp. The men selected for the job, beginning with Kennedy's choice of McGeorge Bundy, have been specialists on national security affairs to a degree that their predecessors under Eisenhower and Truman clearly were not.[95] It is understandable that such an official, so close to the President's thinking and so well informed about the basis and rationale of foreign policy decisions, should be considered an ideal choice to serve as an important communication link and public relations channel to the more alert and informed elements of the public. Performing the additional task of spokesman-apologist for foreign policy need not seriously weaken the Special Assistant's ability to organize the process of policy making and to keep options open. But policy making also includes the sensitive task of reviewing and reconsidering existing policies, and it is in this respect particularly that participation in the public explanation and justification of current policies may inhibit the Special Assistant's performance of his major responsibility for helping to keep options open and to create new ones.

Another duality and potential role conflict can be identified between the custodian task and what may be called the "watch dog" task. The custodian's job is to protect the chief executive's broad responsibility for the quality of the policy-forming process. The "watch dog" task is concerned, more narrowly, with protecting the President's personal power stakes in the sense discussed earlier. "Custodian" and "watch dog" tasks cannot be comfortably combined in one role. The attempt to do so may lead, as some critics have charged, to an erosion of the Special Assistant's performance of his responsibility for maintaining the quality of the policy-making process. When he attempts to perform both functions, the Special Assistant risks losing the capacity for serving as the "honest broker" of ideas and analyses. To serve as guardian of the President's personal power stakes in a contoversial policy matter can easily lead the Special Assistant to become a gate-closer; instead of helping to keep the President's options open, he helps the President to keep them closed. This is the criticism leveled at Walt Rostow's performance in the role.[96] The question has also been raised whether serving as personal adviser to the President has required Kissinger to subordinate his institutional role as kingpin of the NSC system.[97]

Finally, at risk of hopelessly complicating the problem, we must recognize still another role conflict that may affect adversely the Special Assistant's ability to discharge the custodian's task. If, as has been the case in the past, the Special Assistant is also expected to monitor and ensure the *implementation* of policy decisions once they are taken, can he preserve intact his other role as the neutral, nonadvocate custodian of the process of policy making (which of course includes policy evaluation and review)? As Thomas E. Cronin points out, White House aides who might be able to fashion a fairly objective role in the process of pol-

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flicts in the position and tried to give priority to his task of keeping the President's choices open. (*The Presidents' Men,* p. 270). Material on Bundy's alleged tendency to shift from arbiter to advocate on a number of occasions is reported in the highly critical appraisal offered by David Halberstam ("The Very Expensive Education of McGeorge Bundy," pp. 23, 29, 34–35.) Irving Destler's *Presidents, Bureaucrats, and Foreign Policy* helps clarify to some extent the role interpretations and performance of Bundy and his successors. David K. Hall is undertaking a comprehensive study of the development and performance of role tasks associated with the Executive Secretary and the Special Assistant within the NSC system as part of his Ph.D. dissertation in the Political Science Department, Stanford University.

[94] Robert Cutler, pp. 315–316.

[95] A former long-term member of the NSC staff notes that the individuals Eisenhower selected to be his Special Assistant for National Security Affairs "were not, with the single exception of his last appointee, Gordon Gray, experts in foreign affairs or defense policy. They were the President's agents in operating the NSC system, but they were not, by and large, independent sources of power and advice" (Robert H. Johnson, "National Security Council," p. 716).

[96] See, for example, Townsend Hoopes, *Limits of Intervention,* pp. 59–61, 116, 123.

[97] Destler, *Presidents, Bureaucrats, and Foreign Policy,* Ch. 5.

icy formation often become advocates and unrelenting lieutenants for fixed views in the implementation stage.[98] The importance of implementation notwithstanding, I would assume that the Special Assistant's personal involvement in it is likely to create serious constraints on his ability to perform as custodian of the policy-making process insofar as concerns reevaluation of that policy. The question is posed, therefore, whether the two tasks of custodian and implementer, both of which the executive requires, need to be combined in the same person and whether they can be kept sufficiently separate to avoid a dysfunctional role conflict.

I have argued that the Special Assistant, or someone like him elsewhere in the advisory system, should be the custodian of the process, and that this critical task should *not* be combined with becoming (1) a policy adviser-advocate; (2) a public spokesman on behalf of existing policy; (3) a watchdog of the President's personal power stakes in policy issues; or (4) an implementer of policy decisions already taken.

These four additional tasks, I feel, cannot be effectively combined with that of custodian. The attempt to do so will undermine the integrity with which an incumbent can perform the task central to the custodian's role, which is to find ways of maintaining and improving the workings of the policy-making system. This the custodian attempts to do on behalf of the executive by balancing actor resources when necessary, by strengthening weaker advocates, or compensating for their weakness, by bringing in new advisers to argue for unpopular options, by setting up new channels of information so that the President and other policy makers are not dependent on a single channel of information, by arranging for independent evaluation of decisional premises and options that are not receiving objective, competent evaluation within the system, by monitoring the policy-making process for possibly dangerous malfunctions and instituting appropriate corrective action.

This "job description" is, indeed, a composite of some of the most useful tasks performed *on occasion* by incumbents of the office. But these critical tasks have by no means been consistently undertaken as we have seen, on all the occasions on which there was a need for them. It is suggested, therefore, that the best performances of the job in the past now be codified and institutionalized into the Special Assistant's role.

The availability of this kind of pragmatic theory should help to institutionalize and regularize self-correcting mechanisms within the policy-making system. As a result, the kind of timely intervention McGeorge Bundy made in the MLF case to prevent a "breakdown" in the advisory system will, it is hoped, become the rule rather than the exception. As we have seen, what this Special Assistant for National Security Affairs did so well for Johnson in this case, no one did for Truman in November, 1950, when the President should have been alerted to his advisers' agreement that something should be done about the maldeployment of MacArthur's forces despite their reluctance to raise the question. Perhaps one of Truman's assistants would have done so had essential features of the multiple advocacy model been institutionalized in the norms of the system at that time.

Similarly some of the inconsistencies noted in McGeorge Bundy's performance might have been reduced had he incorporated into his role a better understanding of the critical tasks required to "manage" the policy-making process more effectively. His insight and ingenuity in safeguarding the President's options, demonstrated so well in the MLF case, were conspicuously absent in other cases, in part because the role itself was not clearly and consistently defined at that time and because Kennedy did not develop a well-defined policy-making system to which he was willing to adhere consistently himself.

Kissinger's definition of the role suffers from even greater role conflict and overload. As was noted earlier, his functioning as policy adviser and alter ego to the President has become even more prominent with the passage of time. As he has become increasingly drawn into the vortex of presidential decision making, with its special requirements for secrecy and personal loyalty, his ability to serve as custodian-manager of the NSC-centered process has been hampered. The dilemma is acute, and as the job of Special Assistant is now defined, it will not be easy to relieve it.

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[98] Personal communication (1971). Cronin considers this and related problems in his recent study of White House "staffers" and their relations with department officials. " 'Everybody Believes in Democracy Until He Gets to the White House . . .': An Examination of White House-Department Relations," *Law and Contemporary Problems*, 35 (Summer, 1970), 573–625 (published July 1971 at the School of Law, Duke University).

I am very much in agreement, as this paper indicates, with the suggestion made earlier by Cronin that political scientists should give more attention to examining the roles and perspectives of members of the presidential advisory system. (T. E. Cronin, "Political Science and Executive Advisory Systems," in T. E. Cronin and S. D. Greenberg, eds., *The Presidential Advisory System* [N.Y.: Harper & Row, 1969], pp. 321–335.)

Conclusion: Some Caveats About Multiple Advocacy

The case for multiple advocacy has been presented in undiluted form. It remains to acknowledge that, as with all other prescriptive theories, this one too has practical limits and costs attached to it.

Multiple advocacy is not offered as a panacea nor as a blueprint that covers all aspects of policymaking. We have noted that ideological values and various cognitive beliefs of policy makers (what we have called "decisional premises") may sometimes be so firmly and uniformly held as to severely constrain the choice of policy. In these circumstances the way in which policy-making procedures are organized and managed may make little difference so far as the substance and quality of the decision is concerned. Often, however, there are competing values and a variety of decisional premises within the decision-making group which, moreover, are not firmly held; in these circumstances, the workings of the policy-making process may indeed exercise a critical influence on the evaluation and choice of policy.

It would be naive, then, to argue that multiple advocacy (or indeed any other policy making model) can guarantee "good" decisions in every instance. Rather, the case for it must rest on the more modest expectation that even an imperfect system of multiple advocacy will help prevent some very bad decisions. This is not to say that malfunctions of the policy-making system of the kind we have identified always result in major policy errors. But they can.

In stating the case for multiple advocacy, I do not mean to imply that it can or should be rigidly applied in any and all circumstances. I have recognized and emphasized that presidential-level policy initiatives are needed from time to time; they are part of the "balancing" that is required to achieve a more rational consideration of policy. With the accumulation of further experience and research on these matters, perhaps we can gain a better understanding of the scope of this and other prescriptive theories. We need to develop a variety of prescriptive theories and to understand more clearly the uses and limitations of each. As such theories become better understood one may hope that a versatile president will choose among them to fit the changing demands of different situations.[99]

I have tried to convey that an effective system of multiple advocacy is not easily achieved in practice. It is not easy to recruit able persons for all the senior positions in the policy-making system and to provide them with all the resources and staffs they need to become effective advocates—competent, analytically able, skillful in bargaining and persuasion. And, in any case, having the resources for advocacy does not ensure that the actors will actually engage in advocacy of all the options that need to be considered. They may avoid advocating some of these options because they run counter to the bureaucratic interests of their departments and agencies. They may eschew raising other options even if they believe in them for fear of ending up on the "losing side" too often, thereby losing "influence" or tarnishing their "reputations," or expending limited bargaining resources in fruitless or costly endeavors. The perception that problems of this kind were imbedded in the workings of "bureaucratic politics" no doubt contributed to the belief of those who designed the present NSC that a strong centralized policy-making system was desirable.

Quite obviously the structure and management of the policy system would have to be designed to give participants a stake in insuring multiple advocacy. Some things can be done to reduce to more tolerable proportions the tendencies noted. These would include selective recruitment, socialization of incumbents into their roles, management of rewards and punishments, selective employment of multiple advocacy for problems in which these inhibitions are less salient. One of the important tasks and responsibilities of a top-level executive (and surrogates charged with managing the policy-making system) is to define the norms and culture of participants in the system to make them consistent with the requirements of the policy-making model he wishes to employ. There is more latitude in this respect than might be imagined; witness the widely different norms and role definitions Kennedy introduced into his policy-making group in the Bay of Pigs and in the Cuban missile crisis.

While the requirements for effective multiple advocacy are not easily achieved, knowledge of the theory is itself useful. It can sensitize the chief executive and his staff to defects in the way important decisions are being made. In any case, multiple advocacy does not have to work perfectly in order to be valuable. To be sure, it is difficult to specify a *critical threshold* that defines operationally when multiple advocacy is likely to work well enough to make a difference. Such a demand on the theory seems legitimate enough at first glance, but I cannot accept it as an essential requirement for practical purposes. Because the kinds of decisions we are concerned with are subject to complex multiple causation, the notion of a fixed "critical threshold" for the performance of the policy-making process, equally applicable to all cases, is misleading. Rather, the degree of multiple advocacy that is "critical" and may suffice to obtain a more effective decision will depend upon the configuration of the other causal variables, which changes from case to case. In some cases even a modest amount of multiple advocacy may suffice; in other cases even a textbook replica of multiple advocacy at its best will not overcome the thrust of other variables (e.g., ideology, operational code beliefs, limitations of knowledge and information, personality idiosyncracies, etc.) that favor a less effective decision.

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[99] Suggestive in this respect is the systematic effort that management specialists are making to draw on available empirical evidence to specify the set of rules which should be used by an executive in determining the form and amount of participation by subordinates to be used in different classes of situations. (Vroom and Yetton, "Normative and Descriptive Models of Participation in Decision-Making," pp. 8–9; for a fuller statement see these authors' forthcoming book, *Leadership and Decision-Making* [ms.].)

As the foregoing implies, I agree with the observation of some critics that multiple advocacy would tend to be a "fragile, unstable" system. But this would be a more damaging limitation if a certain level of proficiency in multiple advocacy had to be achieved in every case (the "critical threshold" problem). Multiple advocacy strives to provide the functional equivalent of rationality in the policy-making process of a complex organization; something less than that may still be better than the alternative. In judging multiple advocacy one must compare it with some alternative system, not with an ideal standard. No policy-making system looks very good when compared with the ideal.

The chief executive's attitude toward multiple advocacy is, of course, critical. This model of organizational policy making is likely to suit the style and temperament of some presidents more than others. Apart from this, how shall we deal with the observation that multiple advocacy would be bad advice and unwelcome to a president who knew what he wanted to do and who regards his chief problem as that of imposing his decision on subordinates and getting them to implement it? Certainly there will be many occasions on which a president must impose his policy on other actors within the executive branch. The problem of evaluating alternative policy-making models, however, cannot be tied to the assumption that the President's *initial* policy preference is always the most effective and desirable option. It may indeed be "unwelcome" but not therefore "bad" advice to a president who "knows what he wants to do" to expect of him that he subject his preferred option to serious scrutiny and debate. One can hope that a president will see that it is to his advantage to avoid reaching premature closure in his own mind about the best option until the processes of multiple advocacy have had an adequate opportunity to illuminate the issue. Certainly the final choice of policy has to remain the President's. Most everyone agrees that he should have real alternatives from which to choose. But it is not only other actors in the policy-making system who, when bureaucratic politics works badly, can narrow and delimit the President's choice; the President himself can deprive himself of genuine alternatives and a reasoned choice.

The argument may be granted, but the dilemmas of presidential decision making are such that a remedy for one problem may create or exacerbate still another problem. Thus, to submit to multiple advocacy may entail costs that a president would rather avoid incurring. The time required for effective advocacy and for give-and-take among the advocates may on occasion impose undue delays on decision making at the executive level. In a full-blown system of multiple advocacy, competition and conflict may occasionally get out of hand, strain cohesion, impose heavy human costs, create political difficulties for the chief executive in his relations with Congress and the public. Cast in the role of advocates, officials may be quicker to go outside the executive branch in search of allies in policy disputes. The chief executive may feel the weakening of his control over final decisions outweighs the benefits he gets from multiple advocacy.

There is no denying that multiple advocacy entails costs which may be onerous and difficult to live with from time to time. On the other hand, other systems of policy making also entail costs and risks, though different and perhaps less conspicuous. The *absence* of effective multiple advocacy, as we have seen on a number of historic occasions, can also have very serious costs in terms of the maladaptive policies it generates. It is hoped that this formulation of the theory together with the historical documentation of its importance provided here will at least call attention to some of the fundamental problems, lacunae, and defects in other policy-making systems, and, in addition, stimulate efforts to meet the challenge of making multiple advocacy work.

Comment: Multiple Advocacy: Some "Limits and Costs"

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The game of foreign affairs organizational prescription has been played by scores of scholars and study groups since the Second World War. But most efforts by academicians and practitioners alike have been flawed by a tendency to view the bureaucratic world less as it was than as aspiring reformers wanted it to be. Happily, Alexander George's "The Case for Multiple Advocacy" goes a long way toward overcoming this tendency. It is well grounded in social science research about organizations in general and bureaucratic politics in particular. It takes a careful, comparative look at how an important part of the foreign policy decision-making system seems to have actually operated in a series of specific crises. And its reform proposal draws directly on both this research and this review of actual experience. Moreover, George's is one of the few efforts anywhere to describe in some detail the bureaucratic political pattern for foreign policy making that he would promote, including delineation of the roles of the officials on which it depends.[1]

These considerable virtues notwithstanding, I think there is strong reason to doubt that his "multiple advocacy" proposal is workable in the form in which he has advanced it. Moreover, it is open to serious question whether it would make our foreign policy more "rational" —as George has used the term—even if it were bureaucratically viable. It will better serve George's objective—the development of "normative-prescriptive theories of organizational policy making"—if I concentrate here not on aspects of the problem he has handled well, but on important issues he has neglected. So I seek here briefly to argue two general points—

(1) that his "Special Assistant" ("custodian" of the "multiple advocacy" process) is unlikely to be able to develop the leverage he will need to manage the system effectively;

(2) that, even if one grants its viability, the system's net contribution to "rational policy making" might be small, or even negative, unless one can attribute overriding importance to presidential decisions (relative to lower-level decisions and actions) as determinants of United States foreign policy.

George's strategy is built around the Special Assistant, who is charged with "maintaining and supervising the competitive nature of policy-making within the organization." Other roles played by past occupants of this position have tended to conflict with this "custodian" task. Therefore, the Special Assistant would refrain from being a policy "advocate" or "adviser," a "spokesman" on behalf of existing policy, a "watchdog" for the President's power stakes, or an "implementer" of decisions already taken. For all of these would compromise his neutrality by giving him a vested interest in particular policy courses. It is not clear whether George would give to another man all of the Special Assistant's responsibility for coordinating the President's day-to-day "operational" foreign policy business, a task which Eisenhower kept separate from Cutler's role but which has been central to the job of Bundy and his successors. But this responsibility would presumably be very much trimmed, since much of the work falls into categories that conflict with the "custodian" function which George wants played in its purest possible form.

With what sort of power base would this leave the Special Assistant? About this George has little to say, other than to note that "the custodian's role can be introduced and maintained only by the chief executive."[2] And doubtless the Special Assistant will need a strong presidential mandate and continuing presidential support if bureaucrats are to take him seriously. But has not the very range of roles played by recent Special Assistants been an important source of their power? Has not their intimate relationship with the President, the foundation of their bureaucratic influence, been based on the multiple presidential purposes they serve? And has not their leverage over the bureaucracy been likewise enhanced because they can influence a wide range of presidential business—from the list of those invited to White House meetings to the wording of National Security Decision Memoranda (orders), from the papers the President sees (and the form and manner in which he sees them) to the prospect of a White House audience for a particular foreign official or dignitary? Certainly much of Kissinger's strength—and his ability to get bureaucrats to take his formal "options" system seriously—stems from his predominance as operational coordinator and informal presidential staff man and adviser in virtually all phases of the policy-making process. By contrast, the lack of a recognized "operational coordination" role has helped to undercut a new State Department unit whose purpose is to bring a greater range of policy choices to the Secretary's attention—the Planning and Coordination Staff created in 1969.[3]

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[1] For an extended analysis of how (and, in part, why) postwar organizational proposals for foreign policy have generally avoided doing any of these things, see my *Presidents, Bureaucrats, and Foreign Policy: The Politics of Organizational Reform* (Princeton: Princeton University Press, 1972).

[2] In this respect George's prescription shares a flaw common to less sophisticated reform proposals like Nelson Rockefeller's "First Secretary of the Government" and the Herter Committee's "Executive Under Secretary of State": careful attention to defining the role, very little attention to how its occupant will achieve the leverage to play the role effectively.

It can be argued in response that it is hardly necessary, or even desirable, for the proposed "custodian" to become a Kissinger; a considerably lesser order of influence will do. Still, one should not underestimate the amount of leverage the occupant of the position *will* require. He will have primary responsibility for "management of rewards and punishments" to minimize "quasi resolution of conflict" and other forms of "restraint of trade," and "to give participants a stake in insuring multiple advocacy." This will require substantial bargaining resources in the hands of the Special Assistant. So will efforts to strengthen weaker advocates and create new sources of information and analysis. In his efforts to assure "time for adequate give-and-take," as well as timely and explicit consideration of larger policy issues, the custodian will be contesting with high officials who seek to foreclose options by taking issues directly to the President and reaching understandings with him on a one-to-one basis. Nor could his problems be solved by creating a set of formal procedures like Nixon's NSC system. For experience suggests that such formal systems tend to influence important business only to the extent that they are built around officials who have influence independent of the system; otherwise they degenerate into "paper mills."

In the absence of persuasive argument to the contrary, it is hard to avoid the conclusion that the "custodian" would rapidly be reduced to an isolated spokesman for rationality. In response to the day-to-day bureaucratic needs of the President and other high officials, ad hoc decision-making procedures would arise which the "custodian" could neither change nor control.

But suppose that the custodian is able to play his role effectively, and the system is able to operate more or less as it is designed to operate. Would it lead on balance to a more "rational" foreign policy? The conclusion that it would, I believe, depends on the pervasive assumption that American foreign policy is largely controlled by explicit presidential choices such as those George portrays.

George does not explicitly make this assumption. The "empirical base" for his analysis is admittedly "confined to a special (though very important) class of foreign policy decisions," those on "whether to make a commitment, to intervene in, or escalate a conflict." There is no claim that these decisions—or presidential decisions more generally—encompass the whole of the problem of rational foreign policy making. "The importance of implementation" is several times recognized. But it is recognized in a way which tends to minimize its importance. First, it seems to be regarded as something that can be handled in a separate (and presumably less important) system. Second and more important, the very use of the world "implementation" (rather than a more neutral expression such as "lower-level decisions and actions") implies that what officials below the President do is rather narrowly constrained (or can be constrained) by major presidential decisions.[4] This view also seems to have been held by

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[3] Nor can one be reassured by George's reference to Cutler as a Special Assistant with a particularly well-defined and delimited role. His influence seems to have been far less than that which George's "custodian" would require. But it should not be inferred from this discussion that there are easy ways of measuring the various sources of "leverage" available to an official like the Special Assistant, or of predicting whether such a role would be bureaucratically viable. Analysts of such matters—this one included—have not gotten much beyond the stage of making lists of "bargaining advantages," such as the "resources" for advocacy enumerated by George. But this is hardly a case for ignoring the problem—as most organizational reform proposals do.

[4] Graham T. Allison and Morton H. Halperin argue elsewhere:

> The action channel for major foreign policy decisions can be usefully divided into that portion which leads to decisions by senior players and that part which follows from those decisions. The latter is frequently referred to as 'implementation' but we resist that terminology as too restrictive. Many elements of implementation stem from sources other than decisions by senior players.

("Bureaucratic Politics: A Paradigm and Some Policy Implications," *World Politics*, Vol. 24 [Spring Supplement, 1972], p. 46.) Thus, for purposes of analysis, they divide the bureaucratic politics of foreign policymaking into "policy and decision games" and "action games."

Nixon and Kissinger in early 1969, and is reflected both in the explanations of their system which George cites and in the original assignment of "operational coordination" to a committee of subordinate officials. Facilitating "rational and deliberate choice" by the President is what is critical. Problems of operational control of the bureaucracy are hardly dismissed, but they are treated as secondary.

How important in fact are such major presidential decisions as determinants of American foreign policy? We have no way of measuring this at all precisely; conceivably we never will have. Yet there is a range of relevant evidence, testimony, and analysis to suggest that presidential decisions by themselves are much less decisive than many have thought. Kissinger once described his job as being to "help the President set goals," rather than being "operationally oriented."[5] Yet within six months of Nixon's inauguration he was heading a new "Washington Special Actions Group" charged with crisis coordination, and the "Anderson Papers" immortalized his daily preoccupation last December with bringing bureaucratic actions into line with a presidential decision ("tilt in favor of Pakistan") that does not seem to have been very ambiguous. Kissinger himself remarked, after nineteen months in his job, about the considerable time he had had to devote to operational matters.[6] As strong an advocate of "rational policy making" as George Kennan wrote long ago that "what the foreign affairs segment of the Government needs is not primarily an occasional National Security Council paper but intimate day-by-day, hour-by-hour direction . . . It needs . . . to be ridden herd on."[7] Graham Allison's "Governmental Politics Model" of foreign policy making similarly stresses that "management of piecemeal streams of decisions is more important than steady-state choices; making sure that the government does what is decided is more difficult than selecting the preferred solution."[8]

There are considerable grounds for believing, then, that to treat U.S. foreign policy primarily as the direct outcome of *decisions made by the President* is to distort the process considerably. Instead, one might better begin by characterizing U.S. foreign policy as the outcome of a stream of decisions and actions taken by officials in a range of agencies and at several bureaucratic levels. Such a conclusion in itself would not, of course, render George's prescription useless. For certainty a number of presidential decisions appear to have been very important—Nixon's on China, for example, and those in the cases George cites. "Multiple advocacy," he tells us, "is not offered as a panacea nor as a blueprint that covers all aspects of policy-making." Is it not enough to offer help on a phase of the process which is frequently significant, and which seems more susceptible to management reforms (if only because it is centered in one place) than other parts of the process?

The problem is that, as George recognizes, "a remedy for one problem may create or exacerbate still another problem." Specifically, if one assumes that "subpresidential" decisions and actions are very important, one must ask how the proposed multiple advocacy strategy will affect them. Will it make them more "rational," or less so?

Even if one accepts George's general criterion—that the rationality of a decision can be usefully considered as a function of the range of alternatives considered before making it (and the quality of analysis of these alternatives)—we cannot apply it to lower-level decisions and actions without some modification. For another important aspect of rationality is coherence in overall government foreign policy making. So we also want "subpresidential" decisions and actions to be consistent with presidential decisions—both in letter and in spirit—and more generally with presidentially-preferred policy directions however communicated. These two standards, range of choice and coherence, are themselves not entirely consistent with one another. But one can, for purposes of argument, reconcile whatever conflict exists by considering subpresidential behavior as "rational" when it meets one of the following criteria:

1) given reasonably clear presidential wishes, decisions and actions at lower levels that are consistent with such wishes;

2) given unclear presidential wishes, either (a) referral of the issue back to the President with a wide range of alternatives exposed, or (b) decision on the issue by an official (or group of officials) motivated both to consider as wide a range of values and alternatives as

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[5] Brooke Nihart, "National Security Council: New Staff System After One Year," *Armed Forces Journal*, April 4, 1970, p. 29.

[6] "The outsider believes that a presidential order is consistently followed out. Nonsense. I have to spend considerable time seeing that it is carried out and in the spirit the President intended." Quoted by Saul Pett, "Henry A. Kissinger: Loyal Retainer or Nixon's Svengali?" *Washington Post*, August 23, 1970, p. 83.

[7] "America's Administrative Response to Its World Problems," *Daedalus* (Spring 1958); reprinted in Jackson Subcommittee, *Organizing for National Security*, Volume II, 1961, p. 233.

[8] Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Little, Brown, 1971), p. 146.

the President would, and to decide the matter the way he (they) thinks the President would like it decided.[9]

We shall never, of course, meet these standards for "rationality" in lower-level decision-making in anything like a complete way. But the need to move closer to this goal—at least on issues of overwhelming presidential concern—is an important reason that Kissinger's role has ranged so far beyond the "custodian" function that George would prefer. And the relatively limited impact that one presidential aide can have in this regard has led this analyst to argue elsewhere that the President needs a team of agents at several levels in the government to make decisions and fight bureaucratic battles in his behalf.[10]

In his prescription, George would not provide for such "President's men" in the upper echelons of the bureaucracy. Rather, he would encourage high-level actors to develop and press their agency points of view, and would seek to prevent "maldistribution of resources" needed for advocacy so that these actors could carry close to equal weight. He would avoid the often dysfunctional consequences of decentralized bargaining systems, he argues, because with the decision maker's role added, "advocates are no longer competing against each other," but "for the magistrate's attention."

But the issues on which they will differ are far greater in number than one magistrate can resolve. What will happen on matters that cannot get "the magistrate's attention?" All of the "resources" George would equalize for advocacy before the President can also serve to influence decisions and actions taken below him. On these there would be no "centralized coordination and executive initiative," since the only important official in George's system who comes close to sharing the President's broad foreign policy perspective (the custodian) is explicitly excluded from getting involved. Is it not likely that the "multiple advocacy" system would lead, on subpresidential decisions and actions, to the worst forms of bureaucratic political practices and outcomes—minimal compromises, deference to bureaucratic rivals in their fields of expertise, continuation of contradictory courses of action by different agencies—in short the very sorts of things the proposal is designed to prevent?

Greater use of the "collegial" approach might ease the problem. If through this style of advocacy before the President high officials could be encouraged to broaden their perspectives, this might affect the outcomes they seek on subpresidential matters. Another possible approach would be to concentrate on strengthening those "advocates"—the Secretary of State, the Secretary of Defense—who would seem to have relatively broad perspectives (and considerable dependence on the President) built into their roles. Their decisions and actions might therefore be more likely to meet the criteria for "subpresidential" rationality suggested here than those of officials with narrower functional or careerist orientations. Part of this strengthening would necessarily aim at making these Cabinet Secretaries less dependent on—and better able to control—the career services over which they sit. This would presumably mean an Office of the Secretary of Defense comparable in clout to that under McNamara, and an "Office of the Secretary of State" much stronger than any that has heretofore existed.

But the larger problem seems to be that the two goals—"rational" presidential decisions as meant by George and "rational" subpresidential decisions and actions as rather generally defined here—are in important respects conflicting goals. A foreign policy-making system designed with concern for just one of them, then, will probably weaken the prospects for attainment of the other. To cultivate "advocacy" before the magistrate is also to encourage it on matters which cannot be squeezed onto the magistrate's decision agenda. Conversely, there are costs to any effort to build central coordinators or presidential "agents" into the bureaucracy at several levels—e.g., the Secretary of State, Assistant Secretaries—so that they can bring rationality to subpresidential decisions and actions. For to make such men effective the President must create a presumption—well-understood in the bureaucracy—that the President will back up his "agents" when the decisions they make are appealed to him.[11] This cannot but conflict with the magistrate role to which George would have the President give priority.

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[9] Presumably, to be both able to and motivated to decide an issue in such a "presidential" way, an official would need to be in close communication with the President, and to derive much of his own leverage in the bureaucracy from Presidential confidence.

[10] I have also argued that they could better combine responsiveness to the President with influence over day-to-day operations if this team were headed by the Secretary of State and composed mainly of high State Department policy officials. *Presidents, Bureaucrats, and Foreign Policy,* Chs. 5, 9.

[11] Sorensen expresses this dilemma well: "In choosing between conflicting advice, the President is also choosing between conflicting advisers, conferring recognition on some while rebuffing others. He will,

Ideally, a prescriptive theory seeking to "rationalize" foreign policy making would include: (1) some empirically based assumptions about the relative importance of presidential and subpresidential decisions and actions; and (2) proposals for institutions and procedures aimed at maximizing rationality over all government policy decisions and actions. Such proposals, in turn, would be based not only on the above assumptions, but also on some trade-off function relating the two rationality values —how much we lose of one to get more of the other. To put forward such a prescription for prescriptions is hardly to criticize George, since he lacks (as all of us do) the information and the analytical base upon which such a theory might be built. Indeed, the rather primitive way the problem is stated here suggests that we would do well to continue on the general methodological path George has taken—conceptualizing which draws on relevant research and empirical evidence to suggest relationships and illustrate concepts, but which does not pretend it can yet establish very precisely their degree of validity. And if such further analysis tended to affirm the line of argument developed here, it would serve one of the important purposes of George's effort—to clarify the "limits and costs" of multiple advocacy as an approach to foreign policy making.

It would also lead us to conclude, however, that these limits are pretty severe, and that while George's overall analysis is likely to prove very important and influential, his specific prescription in the form presented here will be of lesser value. For any prescriptive effort would seem incomplete if it concentrates on role definition but neglects to show how the key actor can develop the leverage he will need to play the role effectively. Nor, this analyst feels, can we get very far if we accept too readily the characterization of the foreign policy process as decisively influenced by the choices of the President.

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consequently, take care to pay more attention to the advice of the man who must carry out the decision than the advice of a mere 'kibitzer.' He will be slow to overrule a Cabinet officer whose pride or prestige have been committed, not only to ease that officer's personal feelings but to maintain his utility and credibility" (*Decision-Making in the White House* [New York: Columbia University Press, 1963], p. 79).

Rejoinder to "Comment" by I. M. Destler

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In his comment on my paper, Destler provides a double-barreled criticism of multiple advocacy: if desirable, not feasible; if feasible, not desirable. First, by questioning whether a "custodian" of the kind I favor could develop enough leverage to manage the system effectively, Destler implies that even if it is desirable to redefine the role of Special Assistant in this way it could not be performed in a viable fashion, and hence multiple advocacy is not feasible. But Destler attacks it also from another direction: granting that the custodian's role may turn out to be viable and feasible, he then argues that the consequences of multiple advocacy might be so harmful as to make it undesirable. There is no inconsistency between these two criticisms; they add up to the belief that multiple advocacy may be neither desirable nor feasible, and, in any case, may have to be rejected on either ground. At the same time, however, I take comfort from the fact that Destler agrees that the information and analysis necessary to evaluate these matters in a more confident way is presently lacking. Further, we are in agreement that one major function of our "debate" over the virtues of alternative ways of organizing and managing foreign policy making should be to stimulate additional reflection and better focussed research.

Destler's observations deserve serious consideration, since they are grounded in the considerable research and reflection that makes his study, *Presidents, Bureaucrats, and Foreign Policy,* an important as well as a timely contribution. While he confines his criticism to two major matters which admittedly I neglected in my paper, his discussion of them indicates a broad area of disagreement between us. Moreover, disagreement on specifics, I suspect, is rooted more deeply in our differing conceptions of the presidency to which I shall return after discussing his two major points.

(1) Will the Custodian Have Enough Leverage?

Destler anticipated the immediate and major response I would make to this question when he noted that "it can be argued in response that it is hardly necessary . . . for the proposed 'custodian' to become a Kissinger; a considerably lesser order of influence will do."

Beyond this, the question is how much and what kind of "leverage" a Special Assistant would need if his role were confined to that of custodian of the policy-making process. I agree with Destler's observation that the custodian "will need a strong presidential mandate and continuing presidential support if bureaucrats are to take him seriously." I do not agree, however, with Destler's speculative judgment that in practice this means that a custodian may require virtually the same kind of power base that Kissinger has been given. Destler overlooks the fact that the present Special Assistant has acquired unprecedented power and bargaining advantages precisely because the President has wanted to impose upon the departments an unprecedentedly centralized model of foreign policy making and implementation. To repress advocacy in policy making to the extent that this administration has, and to substitute a system of producing "pure," advocate-free options, as Kissinger has attempted to do on behalf of the President, surely requires much more power in his hands than a custodian would need to monitor, encourage, and nourish the workings of multiple advocacy on behalf of a president who wanted to give it more scope.

To be reminded that a custodian, too, will need leverage and that the President must back him up in meaningful ways is useful. But having made this relevant point, Destler proceeds to sketch a "worst case" analysis of how badly such an effort could turn out: ". . . the 'custodian' would rapidly be reduced to an isolated spokesman for rationality." While "worst case" analysis can serve a useful function in alerting the unwary to critical variables and unforeseen obstacles, and while Destler's scenario is plausible under certain assumptions (e.g., the President pays only lip service to multiple advocacy and does very little to support the custodian), the task of analysis is not adequately served by limiting it to the construction of a "worst case" outcome. Destler's scenario is not a convincing or compelling forecast of what would happen if the Special Assistant played the role of custodian for a president who wanted and knew how to use a multiple advocacy system.

Nor is Destler's case strengthened by the observation that the influence of Eisenhower's Special Assistant, Robert Cutler, "seems" to have been far less than a custodian would require in a system of multiple advocacy. The main thrust of Cutler's role, we must remember, was not to improve multiple advocacy but rather to produce "agreed-upon" positions, consonant with Eisenhower's preference for a staff system that produced a single well-considered recommendation. But in practice, of course, Eisenhower and Cutler notwithstanding, advocacy and policy debate often took place, whether before or after the effort to formulate an "agreed-upon" position and whether on Cutler's formal track of policy analysis or outside and around it. That Cutler lacked enough leverage—and perhaps incentive as well—to channel advocacy more efficiently and reliably into NSC procedures for producing "agreed-upon" positions does not speak directly to the question whether his influence would have been sufficient for the quite different task of helping to make a system of multiple advocacy work better.

Nor is there reason to assume that the custodian's procedural role would necessarily be so divorced from the all important "action-forcing" aspects of the policy-making process as to render his activities inconsequential. Admittedly this could happen if the President failed to use his Special Assistant as a conduit of the action-forcing process that he himself initiated on certain policy issues or which departmental and agency actors initiated on other issues. In either case the attitude of the President would generally be decisive in determining whether the Special Assistant was able to serve as a conduit in order to implement multiple advocacy on a given issue. I cannot imagine that any president would behave altogether consistently in this respect. Even if he generally favored a system of multiple advocacy, no doubt he would exercise good judgment to bypass it in some cases and, one has to expect, bad judgment in bypassing it on other occasions. I have already acknowledged in my paper that multiple advocacy would have to work with some degree of flexibility, depending on various conditions, and that it would also work imperfectly as, indeed, do all alternative policy-making systems.

(2) Could Multiple Advocacy be Harmful?

The second line of attack which Destler directs against multiple advocacy challenges its desirability: "Even if one grants its viability," Destler holds, "the system's net contribution to 'rational policy making' might be small, or even negative, unless one can attribute overriding importance to presidential decisions (relative to lower-level decisions and actions) as determinants of United States foreign policy." This second issue is more complex than the first. There are two major strands to Destler's challenge, which I will deal with in turn.

(a) *How Important are Presidential Decisions?*

Destler first questions the need for multiple advocacy by asking whether presidential decisions are really so important in shaping foreign policy. He does not reject the argument I have advanced that multiple advocacy has in the past[1] and is likely again in the future to help avoid some very bad presidential decisions if it were institutionalized and managed more effectively. I read between the lines of Destler's comment an implicit concession that multiple advocacy might well produce decisions that were more "rational" and better than otherwise. If so, I would think an important part of the case for multiple advocacy would have been established.

It is not necessary in my view to suspend judgment, as Destler would have us do, until we can ascertain just how important presidential decisions are in shaping the entire realm of foreign policy. It is also somewhat beside the point in this context that "presidential decisions *by themselves* are much less decisive than many have thought" (emphasis added). The feasibility and likelihood of effective implementation of presidential decisions and policies is indeed, as I noted, an important requirement of a rational decision. But the fact that any presidential decision has to be effectively implemented cannot be used to downgrade the importance of choices which the President makes. Thus in the Cuban missile crisis, for example, we can hardly dismiss the significance of the President's choice of the blockade as against the air strike.

(b) *What is the Effect of Multiple Advocacy at the Presidential Level on Lower Level Decision-Making?*

In questioning the desirability of multiple advocacy, Destler develops a second line of analysis which is more complex and also more weighty. He distinguishes between presidential level decisions and lower-level decisions and actions, postulates that rationality and effective foreign policy require "coherence" between lower-level and presidential decisions, and then argues that multiple advocacy at the presidential level would defeat the requirement of "coherence."

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[1] In addition to the useful role that multiple advocacy played in several cases discussed in my paper (Indochina-1954, MLF, and Cuban missile crisis), reference can be made to a number of other occasions on which multiple advocacy appears to have helped to curb choice of questionable policy options: the Formosa Straits crisis of 1954–55; the Quemoy crisis of 1958; the Laos crisis of 1961.

I agree that subpresidential decisions are important. More than Destler does, however, I would emphasize that many issues cannot and should not be decided at the top decision-making level. It is true that I did not give sufficient attention to the relationship between the use of multiple advocacy at presidential level decision making and ways of making decisions at lower levels. But I did suggest and would emphasize more strongly here the desirability of employing a form of multiple advocacy (or, at least, adversary proceedings) also in making decisions at lower levels. (See also below.) This is part of my answer to Destler's reminder that the President cannot be expected to deal with all the issues that are of concern to the advocates and on which they disagree. It is also part of my answer to his concern lest the pursuit of rationality via multiple advocacy at the presidential level result in uncontrolled bureaucratic politics on other issues at lower levels.

What is involved here, more fundamentally, is a difficult management task which neither I nor Destler clearly stated, namely the chronic problem of deciding *at which level* in the organization *different* issues should be decided. This issue is quite relevant to Destler's discussion of "coherence," which I find inadequate and to which I shall presently return.

Destler incorrectly reads into my proposal that there would be "no centralized coordination and executive initiative" in a system of multiple advocacy since, as he puts it, "the only important official in George's system who comes close to sharing the President's broad foreign policy perspective (the custodian) is explicitly excluded from getting involved." Actually, I recognized the importance of all of the role tasks, not merely that of custodian, that are presently combined in the Special Assistant, and the necessity to assign responsibility for them to other presidential assistants. Needless to say, there would have to be other important members of the President's staff, and I see no reason why they, too, could not share the President's broad perspective.

It will be necessary to develop a more detailed picture than I have thus far of the structure and workings of the President's staff in a system of multiple advocacy, and how this staff would interact with departments and agencies. I have presented my proposal for multiple advocacy as a design concept rather than as a blueprint. Some of Destler's comments are useful in anticipating problems that might arise and would have to be taken into account in drafting a detailed blueprint. But when Destler turns to exploring what the relationship between presidential and lower-level decisions might look like in a system of multiple advocacy he presents once again a "worst case" analysis. Starting from the incorrect observation (and quite unnecessary assumption, to which I have already alluded) that there would be "no centralized coordination and executive initiative" in a multiple advocacy system, etc., he foresees the worst case as emerging: "Is it not likely that the 'multiple advocacy' system would lead, on subpresidential decisions and actions, to the worst forms of bureaucratic political practices and outcomes . . . in short the very sorts of things the proposal is designed to prevent?" The answer to this "parade of horribles" is no, at least not necessarily so—as Destler goes on to recognize. He mentions three ways in which this worst case nightmare might be alleviated if not avoided, the first two of which (greater use of the "collegial" model and a strengthening of two "advocates," the Secretaries of State and of Defense) are indeed central features of my proposal.

Destler's third suggestion for minimizing the severity of his worst case is that presidential assistants be employed to achieve greater "coherence" between presidential level decisions and policies on the one hand and lower-level decisions and actions on the other. While I accept the general idea of presidential assistants to serve as links with lower-level decision making, I have substantial reservations about the way Destler seems to define their function and activities, and the connotations of his requirement of coherence.

"Coherence" can well mean rather different things depending on whether the particular decisions and actions to be taken at lower levels are by way of implementing a specific, concrete policy chosen by the President (e.g., the blockade in the Cuban missile crisis) or whether these decisions and actions are of another kind for which responsibility has been delegated to lower-level officials by the President or given to them by legislative mandate. The meaning and nature of the requirement of "coherence" varies a good bit in these two cases. Destler, however, implies that a uniformly strong constraint for consistency with the President's "wishes" should apply to all lower level decisions and actions. Thus, to paraphrase him only slightly, officials at lower levels should always decide and act as they think (or are led to believe, one might add) the President wishes; they should refer the issue back to the President if his wishes are not clear; or they should decide themselves by attempting to simulate the way they think the President would have decided it.

Stated in so extreme and undifferentiated a way, the requirement of "coherence" becomes questionable. It offers no qualification of the role that presidential "wishes" should play throughout the system and no built-in safeguards against fallibility and error at the presidential level. I cannot accept "coherence," as presently stated, as a necessary addition to, or modification of my general criterion of "rationality." To do so, as Destler suggests, would distort the broader meaning I attempted to give to "rationality" and reduce it to a far narrower criterion of efficiency that is linked with the President's wishes.

Whether he intends it that way or not, Destler's discussion of "coherence" and other aspects of his comment provide what I regard as a dubious rationale for tight-fisted determination and control of foreign policy from above. In this context I have strong reservations about the specific form and implications of his proposal for "President's men," whom he refers to as "a team of agents at several levels in the government to make decisions and fight bureaucratic battles in his behalf." One can subscribe comfortably to this definition only if one approves without reservation the highly centralized NSC model introduced by the present administration. If one is concerned, however, about some of the limitations and risks of so centralized a system (which I mentioned in my paper and on which Destler is silent)[2] and, therefore, would like to consider an alternative system which gives greater scope for diversity and multiple advocacy, then one will want to give a significantly different emphasis to the role these presidential assistants should play.

Thus the need at the presidential level is *not* for assistants who invariably attempt to impose the President's point of view and "will" ruthlessly on lower levels but, rather, as Thomas Cronin has emphasized, assistants who will play the quite different role of *"skilled management mediators,"* assistants "who will not be afraid occasionally to widen the scope of conflict." The need for something like regulated multiple advocacy at levels below that of top-level presidential decision making is clearly recognized and stated, moreover, in the conclusion Cronin draws from his detailed study of the role and performance of White House staffers in the past ten years: ". . . the White House is in great need of decisive executive branch mediators who can, with the full confidence of the President, preside over the thorniest of complicated claims and counter-claims by competing cabinet members and know when worthy and important elements of a debate are being seriously neglected or misrepresented within these cabinet level negotiations."[3] Again, as Cronin observes, the White House should take more seriously the need to create "an effective system of multiple and critical advocacy for the substantive and operational aspects" of policy making at the departmental level.[4]

Underlying our "debate" over the desirability and feasibility of multiple advocacy, a debate not fully joined and certainly not resolved, I suspect, are competing concepts of the presidency. To me it would appear that Destler subscribes to a concept of the presidency that seemed compelling to many leading political scientists and other observers as recently as a decade ago but which experience has increasingly called into question. I refer to what appears now to have been an exaggerated image of "presidential competence and beneficence," a concept and expectation that developed in response to historical circumstances of an earlier era and assumed something of the character of doctrinaire orthodoxy.[5] It is not possible to comment in detail on this large issue, which affects not merely foreign policy but other policy areas as well. We are in the midst of a strong general trend toward greater institutionalization and further strengthening of the power of the presidency, which the present administration has done much to further. In recent years presidents (and some governors) have increasingly attempted to deal with clusters of major public policy issues by creating additional structures within their own offices or centralizing and strengthening existing parts of their staffs, generally at the expense of departments and agencies within the executive branch. Apart from the question of whether this trend can or should be reversed, it is still necessary to consider how the contribution that departments and agencies can make to policy analysis and policy innovation might be retained and, indeed, strengthened.

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[2] It is unfortunate that space limitations prevented Destler from commenting on most of the key issues I raised with regard to the present NSC system: (1) the costs of so highly centralized a system; (2) the limitations of its search for "pure," advocate-free options; (3) the risks of the severe role conflict imbedded in the present Special Assistant's position; (4) the possibility that so tight and centralized a presidential control over foreign policy making in the executive branch pushes it in the direction of a more "closed" system and further strengthens the President vis-à-vis Congress.

Because Destler has not stated his views on these key issues, the basis for his apparent support of the present NSC concept is not entirely clear. One possibility is that Destler believes that I have grossly exaggerated the costs and limitations of the present NSC. But, alternatively, his silence on these issues could signify that he believes that its costs and limitations are acceptable because the disadvantages of conceivable alternative systems are assessed by him to be even greater.

[3] T. E. Cronin, "'Everybody Believes in Democracy Until He Gets To The White House . . .': An Examination of White House-Departmental Relations," *Law and Contemporary Problems,* Vol. 35, No. 3 (Summer, 1970), published July, 1971; School of Law, Duke University, p. 622–3.

[4] Cronin, p. 624.

[5] See the excellent critique provided by Thomas E. Cronin, "The Textbook Presidency and Political Science," APSA paper, Los Angeles, September 1970 (reprinted in the *Congressional Record,* October 5, 1970; Vol. 116. No. 174); also, William G. Andrews, "The Presidency, Congress, and Constitutional Theory," APSA paper, Chicago, September, 1971.

Returning to the specific focus of our discussion, I recognize, with Destler, the need for strengthening presidential leadership and direction of foreign policy. But there are different conceptions of leadership and different means of exerting it. To reduce this question to a narrow preoccupation with the problem of presidential control over foreign policy making with the executive branch ignores the prior and more important question of finding ways of improving the quality of the President's decisions and choices.

I believe it is unrealistic to expect major changes in the present NSC and in the definition of Kissinger's enormous and complex role so long as the present administration is in office. Even so, should President Nixon be reelected, one may hope that ways might be improvised to deal with some of the apparent weaknesses and limitations of the present NSC to which Destler and others as well as I have called attention.

My inquiry, however, is broader in scope and has a longer time perspective. One used to speak of the "evolution" of the NSC since its inception in 1947. A more correct appraisal of its history, I believe, would have to stress the significant changes in the NSC and in the foreign policy-making system that each new president has introduced in order not merely to learn from the experience of his predecessors but also to create a system more congenial to his own style of decision making. As a result, to a surprising degree the NSC has lacked continuity from one administration to another.

We may question, therefore, whether the present NSC system is likely to survive—or, even, can possibly survive—a change of administrations. From this perspective it is not sufficient to limit inquiry to ways of improving the Nixon-Kissinger system. The additional question needs to be asked whether their system can be institutionalized and work as well for future Presidents and future Special Assistants. I am inclined to the view that it cannot survive the two very special individuals, Nixon and Kissinger, on which it rests. If this view is correct, then there will be a need, sooner or later, to consider not only alternatives to the present system that a new administration my find attractive but also those alternatives that informed political scientists and specialists on policy-making systems consider desirable and feasible.

Positivism, Historicism, and Political Inquiry*

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> Historicism has developed into an intellectual force of extraordinary significance; it epitomizes our *Weltanschauung* (world view). The historicist principle not only organizes, like an invisible hand, the work of the cultural sciences (*Geisteswissenschaften*), but also permeates everyday thinking.
>
> Karl Mannheim[1]

Recent controversy in American political science about the nature of political inquiry reflects an older and deeper conflict at the level of epistemology between rival theories of knowledge. It is generally recognized that the "behavioral revolution" brought into ascendancy an approach to political inquiry whose epistemological roots lie in positivism and, ultimately, in classical British empiricism. In the 1950s and early 1960s the major theoretical opposition to behavioralism came from writers whose own views about the nature of human knowledge were more or less in agreement with the epistemological tradition deriving from Plato and Aristotle. But what of the opposition to behavioralism that developed in the late 1960s, producing what David Easton has called "the post-behavioral revolution"?[2] Does it give expression to an epistemological position that opposes modern empiricism? Easton sees the difference between behavioralism and the viewpoint of its most recent critics as one more of mood or emphasis than of principle. I shall argue, however, that the recent antibehavioral protest stands in fundamental opposition not only to the approach that behavioralism takes to political inquiry but also to its basic assumptions about the nature of human knowledge.

The importance of a theory of knowledge within the discipline of political science does not always correspond to its importance at the time in the broader field of philosophy. It is fair to say that the political scientists who fashioned the behavioral approach vastly overestimated the standing of logical positivism or logical empiricism both in philosophy generally and in the philosophy of science in particular. By 1950, positivism was virtually dead as a philosophical movement. It had come under strong attack even in the philosophy of science. The leading theory of knowledge by this time was one whose foundations lie in the work of Kant and Hegel or, more precisely, in the radicalization of the Hegelian tradition which occurred in the latter half of the nineteenth century. The transformation of Hegelianism led to a distinctive conception both of the world and of human knowledge. The world, or nature, came to be understood in terms of flux, change, or becoming rather than fixity, permanence, or being. Knowledge was now conceived in terms of creation rather than discovery. Worldviews and theories were seen as individual or social creations, which are shaped decisively by subrational forces. It was denied that the human mind can grasp the character of "reality" or "nature" in any final, objective, or absolute way. The position which I have described was developed comprehensively by Friedrich Nietzsche in the 1800s. Yet a number of other thinkers at that time and in the decades that followed came to similar conclusions, often independently of the influence of Nietzsche or of each other. This position was expressed in a distinctively American idiom in the pragmatist movement, particularly by John Dewey and George Herbert Mead.[3] It was developed in England by F. C. S. Schiller, R. G. Collingwood, and, in his later years, Ludwig

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* This is a revised version of a paper presented at the 1970 Annual Meeting of the American Political Science Association.

[1] *Essays on the Sociology of Knowledge,* ed. Paul Kecskemeti (London: Routledge and Kegan Paul, 1952), p. 84. The lengthy essay from which this quotation is taken is entitled "Historicism."

[2] "The New Revolution in Political Science," *American Political Science Review,* 63 (December, 1969), 1051–1061. This address is reprinted in David Easton, *The Political System,* 2nd ed. (New York: Knopf, 1971), pp. 323–348.

[3] For recent discussions of the development of American pragmatism from Peirce to Mead, see H. S. Thayer, *Meaning and Action* (Indianapolis: Bobbs-Merrill, 1968) and Charles Morris, *The Pragmatic Movement in American Philosophy* (New York: Braziller, 1970). For Dewey, see especially his *Reconstruction in Philosophy* (New York: New American Library, 1950), *Logic: The Theory of Inquiry* (New York: Henry Holt, 1938), and *The Quest for Certainty* (New York: Capricorn Books, 1960). For discussions of Dewey's theory of knowledge which bring out its relativistic aspects, see particularly the essays by John Herman Randall, Bertrand Russell, and Arthur E. Murphy in *The Philosophy of John Dewey,* ed. Paul Arthur Schilpp (Evanston: Northwestern University, 1939). This volume also contains an interesting rejoinder by Dewey. A valuable collection of Mead's writings is contained in Anselm M. Strauss, ed., *The Social Psychology of George Herbert Mead* (Chicago: University of Chicago Press, 1956). See also John W. Petras, ed., *George Herbert Mead: Essays in his Social Philosophy* (New York: Teachers' College Press, 1968).

Wittgenstein.[4] In France, Henri Bergson moved in this direction, followed some decades later by Jean-Paul Sartre and Maurice Merleau-Ponty.[5] In Germany, where it came to be known as "historicism," this position gained wide acceptance through the influence of Nietzsche, Wilhelm Dilthey, Oswald Spengler, Karl Mannheim, and, eventually, Martin Heidegger.

This new theory of knowledge had a profound influence on American historians after the turn of the century, especially on those who wrote about the history of political theory. Easton, in his early writings, assigns to it a large share of the blame for the decline of modern political theory.[6] The behavioral movement in American political science can be seen as an effort to preserve against historicism a sphere in which theoretical knowledge would be possible. Behavioralists conceded that opinions about values are relative to time and place, but argued that objective knowledge about what actually happens in political life can be established through the methodology of modern science. The empiricist tradition, as restated by the Vienna Circle, seemed to offer a secure foundation for a true science of politics. Yet even as the behavioral movement gained momentum, one could anticipate that the epistemological arguments which had been advanced against positivism generally would be directed also against this late manifestation of positivism. That assault has begun. The most recent protest against behavioralism, as distinguished from the older protest in the name of traditional political philosophy, seems to draw its principles from the ascendant theory of knowledge.

I wish to examine the recent debate about the nature of political inquiry in light of the deeper conflict in epistemology, now almost a century old, between positivism and its principal foe. A major difficulty in this undertaking is to decide on a name for the relativistic theory of knowledge that stands in opposition to positivism. Among the terms that might be applied to it are "perspectivism," "subjectivism," "relativism," and "instrumentalism." I shall employ the more widely-used term "historicism," which grows out of German epistemological debate. The term "historicism" has been given a variety of meanings since it became a part of German academic debate late in the nineteenth century. Friedrich Hayek and Karl Popper have used it primarily in a methodological sense to denote the view that the main task of the social scientist is to discover the laws by which whole societies develop and, on the basis of these laws of historical development, to make predictions about the future. A few years earlier the historian Friedrich Meinecke, in a work entitled *Die Entstehung des Historismus* (1936), had used the term in referring to the emphasis, originating with certain eighteenth-century writers, on the singular or unique character of all historical phenomena. I shall be using the term in an epistemological sense to denote the view that all human knowledge is essentially relative to time and place. This seems to have been the principal meaning of the term since the great debate about historicism in Germany in the early decades of the twentieth century.[7]

Historicism is a far more potent and pervasive force today than a half-century ago when Mannheim wrote the sentences which I have taken as an epigraph. Yet surprisingly, it tends to be less visible or manifest now, perhaps because of its very pervasiveness, than it was in Mannheim's time. In the early decades of the century, when historicism was struggling for acceptance against other theories of knowledge, its nature and significance were widely understood. After it vanquished its opponents and became the commonplace doctrine, it tended to recede from sight. Even the term "historicism" has become obscure in its meaning, while no alternative name has gained general acceptance for the epistemological position that this term once designated.

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[4] Schiller's position is discussed in Thayer, *Meaning and Action*, pp. 273–303. For Collingwood, see especially *An Autobiography* (London: Oxford University Press, 1939) and *The Idea of History* (New York: Oxford University Press, 1951). Collingwood's historicism is treated by Leo Strauss, "On Collingwood's Philosophy of History," *Review of Metaphysics*, 5 (June, 1952), 559–586. I discuss Wittgenstein later in this essay.

[5] Of importance here are Bergson's views on intelligence and intuition as approaches to knowledge of reality. See Bergson's *Creative Evolution*, trans. Arthur Mitchell (New York: Henry Holt, 1913), pp. 98–185. Bergson's theory of knowledge is treated by Jacques Maritain, *Bergsonian Philosophy and Thomism*, trans. Mabelle L. Andison (New York: Philosophical Library, 1955) and by William James, "Bergson and his Critique of Intellectualism," in *A Pluralistic Universe* (New York: Longmans, 1909), pp. 225–273. For Bergson's relation to James and American Pragmatism, see Ralph Barton Perry, *The Thought and Character of William James* (Boston: Little, Brown, 1936), II, 599–636. For Sartre, see especially *Being and Nothingness*, trans. Hazel E. Barnes (New York: Philosophical Library, 1956). For Merleau-Ponty's theory of knowledge, see especially *The Phenomenology of Perception*, trans. Colin Smith (New York: Humanities Press, 1962) and *Signs*, trans. Richard C. McCleary (Evanston: Northwestern University Press, 1964). See also Thomas Langan, *Merleau-Ponty's Critique of Reason* (New Haven: Yale Press, 1966) and Albert Rabil, Jr., *Merleau-Ponty: Existentialist of the Social World* (New York: Columbia Press, 1967). Lengthy discussions of both Sartre and Merleau-Ponty, along with bibliographies, are contained in Herbert Spiegelberg, *The Phenomenological Movement*, 2 vols.; 2nd. ed. (The Hague: Nijhoff, 1969).

[6] See "The Decline of Modern Political Theory," *Journal of Politics*, 13 (February, 1951), 36–58; and *The Political System* (New York: Knopf, 1953).

[7] For a concise discussion of the history of this term, along with a useful bibliography, see Maurice Mandelbaum, "Historicism," in *Encyclopedia of Philosophy* (New York: Macmillan and The Free Press, 1967), IV, 22–25. See also Mandelbaum's *The Problem of Historical Knowledge* (New York: Harper and Row, 1967) and Ludwig Landgrebe, *Major Problems in Contemporary European Philosophy*, trans. Kurt F. Reinhardt (New York: Frederick Ungar, 1966).

Epistemological Issues That Divide Positivism and Historicism

The philosophers who belong to the positivist tradition have shared a deep admiration for modern empirical science and a desire to serve its advancement. Their theories of knowledge are designed to support the method of modern science or at least to conform to it. If we go back to Hume, who was identified later by positivists as the chief precursor of their movement, we find that his analysis of human understanding was intended both as an application of the new experimental method of Bacon and Newton and as an epistemological justification for it. More recently, logical positivists have defined the task of philosophy as the logical analysis of scientific concepts, statements, and explanations rather than as the investigation of the epistemological foundations of science, but the soundness of these foundations is presupposed by the analysis.

It is helpful to begin a reconstruction of positivist epistemology by considering the view of modern science that emerges in the positivist literature. One commonly finds a distinction between the empirical sciences and the nonempirical disciplines of logic and pure mathematics. The empirical sciences are distinguished by their effort to formulate in general terms the conditions under which events occur in the world. These general principles will serve as the basis for explanation and prediction. The concepts, laws, and theories of empirical science are said to be different from those of other accounts of the world, such as metaphysics, because they conform to the world as men can know it from observation. Scientific statements are tested by empirical evidence, that is, by the facts of experience as they are accessible to all competent observers. Inasmuch as scientific principles have an objective basis in sensory experience, they can be true independently of time, place, and circumstance, although they are always subject to revision in light of subsequent experience.

Given this view of science, the critical task for a positivist epistemology is to uphold the assumption that our generalizations about the external world are reliable if and only if they are constructed from or tested by the raw material of experience. Accordingly, positivists have typically argued that perception makes us directly aware of something "given," something which has not yet been affected by our judgments. Furthermore, these given data are taken (individually or in combination) to represent the facts of the external world. Positivists would come to disagree as to just *what* is given in experience, e.g., private sense-data or representations of the objects and events of common sense, but they would largely agree that we directly perceive *something*, some particulars, prior to our conceptualization and reasoning. These data serve as the ingredients of reasoning without being essentially transformed by it. Our general notions or concepts are abstracted or constructed from these primitive and unconceptualized data. This epistemological viewpoint asserts further that the human mind, when it operates normally or naturally, perceives the same things in the same way regardless of time and place. It is for this reason that knowledge based on experience has an "objectivity" that no other claims to knowledge can possess. In order to be meaningful, our statements about the external world must be tested against the data of experience. This requirement is embodied, for example, in the famous "verifiability principle" of logical positivism, which holds, in its loosest form, that no statement is factually meaningful unless it can, at least in principle, be shown to be true or false, or rendered probable, by reference to empirical observation.[8]

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[8] An examination of the epistemological foundations of positivism and its conception of science should begin with classical British empiricism, especially with Hume's *Treatise of Human Nature.* I discuss Hume's importance for positivistic approaches to the study of man and society in "Hume's Contribution to Behavioral Science," *Journal of the History of the Behavioral Sciences,* 7 (April, 1971), 154–168. The development of logical positivism out of the logical atomism of Russell and Wittgenstein is discussed by J. O. Urmson, *Philosophical Analysis* (Oxford: Clarendon Press, 1956). A primary source for the epistemological foundations of logical positivism is Moritz Schlick's *Allgemeine Erkenntnislehre* (Berlin: J. Springer, 1918). This work and subsequent epistemological developments in the positivist movement are discussed by A. J. Ayer in his "Introduction" to *Logical Positivism* (Glencoe, Ill.: Free Press, 1959) and by Herbert Feigl, "The Origin and Spirit of Logical Positivism," in Peter Achinstein and Stephen F. Barker, eds., *The Legacy of Logical Positivism* (Baltimore: The Johns Hopkins Press, 1969), pp. 3–24. For Rudolf Carnap's development from the time of his early work, *Der logische Aufbau der Welt* (Berlin: Weltkreis Verlag, 1928), see

Whereas positivism grows out of classical British empiricism, historicism stems from the revolution in philosophy that was initiated by Kant and carried forward in the nineteenth century by such German thinkers as Hegel, Marx, and Nietzsche. Historicism, as it emerged in Germany, proved in the long run to be more vital and influential than similar movements in other countries. At the end of World War I there were three principal schools of philosophy in Germany: Dilthey's "philosophy of life" (*Lebensphilosophie*), the neo-Kantianism of the school of Marburg, and phenomenology. Dilthey had died in 1911, but his school continued to be recognized even in the 1930s as one of the principal movements in German philosophy. Dilthey was looked upon as the spokesman for a highly relativistic form of historicism. In addition to his immediate disciples, who are not widely known today, Dilthey had considerable influence on such philosophers as Jaspers, Heidegger, and Ortega y Gasset and on such sociologists as Troeltsch, Max and Alfred Weber, and Mannheim.[9] Turning to the Marburg school, we find that its most prominent representative after 1920 was Ernst Cassirer. Yet whereas the founders of Marburg neo-Kantianism, Hermann Cohen and Paul Natorp, were not historicists, Cassirer developed a "philosophy of culture" that has much in common with the position of Dilthey.[10] The phenomenological movement at the outset was opposed to historicism. Edmund Husserl, the father of phenomenology, had spoken out strongly against Dilthey's historicism in an essay published in 1911, characterizing it as an "epistemological mistake" that destroys the very notion of objective validity and leads to an extreme relativism. Yet in Husserl's later writings, one finds that even this early opponent of historicism comes increasingly to emphasize the historicity of the mind. Heidegger was the great figure in the merger of phenomenology and historicism.[11]

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the various essays in Paul Arthur Schilpp, ed., *The Philosophy of Rudolf Carnap* (La Salle, Ill.: Open Court, 1963). There are selections from Carnap in Ayer's *Logical Positivism*. Ayer, a student of Schlick and Carnap, deals with epistemological issues in *Language, Truth, and Logic* (New York: Dover, 1952), *The Foundations of Empirical Knowledge* (London: Macmillan, 1940), and *The Problem of Knowledge* (London: Macmillan, 1956). For general accounts, see Victor Kraft, *The Vienna Circle*, trans. Arthur Pap (New York: Philosophical Library, 1953) and Joergen Joergensen, *The Development of Logical Empiricism* (Chicago: University of Chicago Press, 1951). Recent works in the philosophy of science that stand in the positivist tradition include Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace and World, 1961), and Carl Hempel, *Aspects of Scientific Explanation* (New York: Free Press, 1965).

[9] For the position of Dilthey and his school in German philosophy in the early decades of this century, see Werner Brock, *An Introduction to Contemporary German Philosophy* (Cambridge, University Press, 1935). Only a few of Dilthey's writings, collected in twelve volumes as *Gesammelte Schriften* (Stuttgart and Göttengen: Teubner, 1957–1960), have been translated into English. For useful accounts of Dilthey's thought, see, in addition to the aforementioned works by Mandelbaum and Brock, H. P. Rickman, ed., *Pattern and Meaning in History* (New York: Harper, 1962); H. A. Hodges, *Wilhelm Dilthey: An Introduction* (London: Routledge and Kegan Paul, 1944); and H. A. Hodges, *The Philosophy of Wilhelm Dilthey* (London: Routledge and Kegan Paul, 1952). Karl Löwith treats Dilthey's relationship to the Hegelian tradition and his differences from Hegel in *From Hegel to Nietzsche*, trans. David E. Green (New York: Holt, Rinehart and Winston, 1964), pp. 60–65, 120–127.

The writings of Oswald Spengler may be considered in this context. Though not of Dilthey's school, Spengler depicts culture and thought as manifestations of "life" and gives to one of his own works, *Der Mensch und die Technik* (München: Beck, 1931), the subtitle: "Beitrag zu einer Philosophie des Lebens." This work is translated by Charles Francis Atkinson as *Man and Technics: A Contribution to a Philosophy of Life* (New York: Knopf, 1932). We learn from Spengler's correspondence that his work was admired by such members of Dilthey's school as George Misch and Ortega y Gasset. See *Letters of Oswald Spengler: 1913–1936*, trans. and ed. by Arthur Helps (New York: Knopf, 1966), pp. 67–68, 72–74, 102, 194–195, 198–199, 317. Spengler popularized important ideas of Nietzsche, such as the will to power and the relativity of truth. He held that there are no eternal truths, even in mathematics. Every philosophy is an expression of the spirit of its age. See especially the Introduction to *The Decline of the West*, trans. Charles Francis Atkinson, 2 vols. (New York: Knopf, 1926–1928). The first volume of this work, whose German title is *Der Untergang des Abendlandes*, appeared in 1918. It was awarded a prize by the Nietzsche Archive in 1919. In 1924, Spengler delivered an address at the Nietzsche Archive commemorating Nietzsche's eightieth birthday. This address, entitled "Nietzsche and his Century," is contained in Oswald Spengler, *Selected Essays*, trans. Donald O. White (Chicago: Regnery, 1967), pp. 179–197. Useful appraisals of Spengler's work are presented by H. Stuart Hughes, *Oswald Spengler: A Critical Estimate* (New York: Scribner's, 1952) and Anton M. Koktanek, *Oswald Spengler in Seiner Zeit* (München: Beck, 1968).

[10] For Cassirer's theory of knowledge, see *The Philosophy of Symbolic Forms*, trans. Ralph Manheim, 3 vols. (New Haven: Yale Press, 1953–1957) and *The Problem of Knowledge*, trans. William H. Woglom and Charles W. Hendel (New Haven: Yale Press, 1950). Useful essays on Cassirer, along with a full bibliography of his voluminous writings up to 1946, are contained in Paul Arthur Schilpp, ed., *The Philosophy of Ernst Cassirer* (Evanston: The Library of Living Philosophers, 1949). Of particular interest in connection with our present problem are the essays by Helmut Kuhn, Fritz Kaufmann, and David Bidney.

[11] Husserl's critique of Dilthey's historicism is contained in his work entitled "Philosophy as a Rigorous Science," which is reprinted in *Phenomenology and the Crisis of Philosophy*, ed. Quentin Lauer (New York: Harper and Row, 1965). For the historicist tendencies in Husserl's own work, see, in addition to

Historicism, as a theory of knowledge, typically contradicts positivism at the following points:

(1) *No direct awareness of pure sense-data.* Positivism has tended to assume that the data of sensation are present to consciousness in their original or pure form and that these data correspond in some manner to the facts of the external world. Experience is prior to concepts and theories or, in the language of Hume, impressions are prior to ideas. Sense-data are the "given" from which concepts are abstracted and by which theories are tested. It is assumed that we can reach an unbiased view of nature by adhering to experience. Historicism, by contrast, is distinctively Kantian in its interpretation of sense perception. Kant had argued that sense-data are inaccessible to consciousness in their original or pure state. We apprehend them only as unified and structured by *a priori* principles or categories of the mind. Historicism agrees with Kant that the mind is an active and creative agent in the process of cognition and not merely a passive recipient or register of sensations. The experience of which we are aware has already been selected and shaped by the mind itself. We cannot assess our fundamental concepts by working our way back to pure experience because these concepts are already presupposed in experience as it presents itself to consciousness. By the same token, we can never determine if the mysterious source of our experience corresponds to our picture of the objective world. For reasons that will emerge presently, the data of experience are assumed to undergo a somewhat greater transformation in our reflections about man and society than in our reflections about the physical world.

(2) *The historicity of the human mind.* In holding that our perceptions are known to us only as transformed by the mind, historicism takes the side of Kant against empiricism. Yet Kant was not an historicist. He agreed with Hume that the principles of the understanding are natural, i.e., that they are constant from one epoch or society to another. Historicism follows Hegel in asserting that the very ordering principles or categories of the mind have varied with the succession of epochs and cultures. Historical inquiry discloses that there is no single view of the objective world but instead a variety of perspectives or worldviews. Hegel had understood this diversity in terms of the working out of an internal logic of reason. Later historicists have preferred to explain it in terms of the adjustment of men and groups to life under different historical and social conditions. Each epoch or culture develops a characteristic view of the world in its totality; and essential differences will be found from one epoch or culture to another in the presuppositions, values, and categories upon which cognition is based. Historicists have disagreed as to whether all thought and perception within a given society must conform to the prevailing worldview or whether creative individuals can free themselves from prevailing assumptions and project an independent view of reality. There is agreement, however, that all knowledge is perspectival in character and arises not so much from discovery of the real character of nature as from social or individual creation.

(3) *The relativity of truth.* Philosophical or scientific inquiry was traditionally understood as a quest for truth, for final and definitive insight, about the nature of the whole of which man is a part or at least about the nature of man himself. By insisting on the historicity of the human mind, historicism calls into question the very possibility of knowledge that is true or "objective" in the sense of grasping nature as it really is. Whatever "truth" might mean for historicism, it cannot mean the congruence in some sense of thoughts and things. If the experience by which we interpret the world and ourselves enters consciousness only after its transformation by presuppositions or categories of thought which are themselves essentially variable and arbitrary, then all claims to absolute knowledge must be regarded as baseless in principle. Writers in the historicist tradition have found different ways of dealing with the epistemological relativism that is inherent in their position. Hegel and Marx attempted to avoid relativism by teaching that the succession of historical epochs reflects a necessary development toward a final epoch, whose viewpoint

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the works cited in Stanley Rosen's *Nihilism* (New Haven: Yale Press, 1969), pp. 103–104, the following sources: Jacob Klein, "Phenomenology and the History of Science," in *Philosophical Essays in Memory of Edmund Husserl*, ed. Marvin Farber (New York: Greenwood Press, 1968), pp. 143–163; and Robert Welsh Jordan, "Husserl's Phenomenology as an 'Historical' Science," *Social Research*, 35 (Summer, 1968), 245–259. The first volume of Spiegelberg's *The Phenomenological Movement* contains a detailed discussion of Husserl and Heidegger along with extensive bibliographies. For Heidegger, see especially *Being and Time*, trans. John MacQuarrie and Edward Robinson (New York: Harper, 1962). The issue of *The Southern Journal of Philosophy* for Winter, 1970 (Vol. 8, No. 4) is devoted entirely to analyses of Heidegger's thought by leading scholars. Heidegger's historicism is treated as such by Helmut Kuhn, *Encounter with Nothingness* (Hinsdale, Ill.: Henry Regnery, 1949) and Stanley Rosen, *Nihilism*. See also P. Christopher Smith, "Heidegger's Critique of Absolute Knowledge," *The New Scholasticism*, 45 (Winter, 1971), 56–86.

will represent a true account of the historical process as a whole. Since Nietzsche, however, historicists have generally denied that the succession of cultures reflects an inner necessity or rationality. They have largely agreed with Nietzsche that the modern epoch is distinguished not by the finality or absoluteness of its view of man and the world but rather by its insight into the essential relativity of all such worldviews. If any final truth is to be admitted, it is the principle that no final truths about the nature of things are possible. There is no possibility of a supratemporal truth which can be grasped from above the historical stream.

It is clear that historicism challenges the claims that positivism has advanced on behalf of modern science, but the full extent of that challenge has only recently become evident. American pragmatism represented itself as favorable to the advancement of natural and social science; while early German historicism, though strongly opposed to the effort of positivists to impose the method of natural science on the human studies (*Geisteswissenschaften*) typically conceded that the scientific method is authoritative in the study of natural phenomena. Historicists such as Dilthey and Mannheim argued as follows: The method of natural science is well suited for discovering and explaining regularities in the external relations of physical phenomena, but not for understanding the observable manifestations of human life. All human expressions, whether deeds, artifacts, symbols, or theories, have meaning for the persons who create them. They embody their creators' experience of life, their feelings, insights, and aspirations. If we analyze these expressions as we might analyze physical objects, looking only at their outward appearances and relationships, we can grasp their "objective meaning," to use Mannheim's terminology, but not their "expressive meaning," the meaning they have for their authors. In order to gain this inner meaning of human expressions, the observer must enter sympathetically into the life from which they arose and relive the experiences which they express. This is possible through the method of understanding (*Das Verstehen*), which must be the principal method of the human studies. Human expressions have still another type of meaning that must be grasped—what Mannheim calls their "documentary meaning." All human expressions point beyond themselves to the characteristic worldview (*Weltanschauung*) of the epoch or culture to which they belong. This underlying impulse or spirit makes the culture a whole and determines the shape of all thought and evaluation within it. We grasp the documentary meaning of human objectifications by seeing them as unconscious expressions of a worldview. Even theoretical philosophy is but a channel through which the spirit of the age finds expression. In comparing the natural sciences and the human studies, the early historicists concluded that the discoveries of natural science develop cumulatively and achieve a timeless validity because they deal only with externalities. The historical setting of the scientific observer, his aspirations and values, need not affect his judgments. Yet the perception of meaning in human expressions requires interpretation and understanding on the part of the investigator, which will necessarily be shaped by his particular worldview. His historical perspective will be evident in the very categories of meaning he uses, in his principles of selection, and in his evaluations. The spirit of his own age must color his interpretation of the human things; and since no worldview can claim an absolute validity, his interpretation must be temporal and relative.[12]

The early historicists separated the natural sciences from the human studies in speaking of the relativity of knowledge, but one could argue on historicist grounds that any application of the scientific method, even in the area of natural science, must be relative to the scientist's perspective or worldview. One might thus hold that even the theories and interpretations of natural science rest on presuppositions about man and the world that are historically variable and, in the final analysis, arbitrary and irrational. As we shall now see, this reinterpretation of natural science in accordance with historicist principles has been in progress in the philosophy of science for more than a decade.

The Revolt Against Positivism in the Philosophy of Science

Thus far we have been concerned with the most general issues of epistemology that have arisen in the struggle between positivism and historicism during the past century. It is in light of this struggle over epistemological principles, I believe, that we must understand the origin and significance of the cleavage that has developed recently within the philosophy of science regarding the nature of scientific inquiry. Logical positivism was formulated in the 1920s with the express aim of remaking philosophy in conformity with what was presumed to be the logic and epistemology of modern natural science. For the next three decades the most influential statements on the nature of science came from members of the Vienna Circle or their followers. From the beginning there were those, such as Karl Popper, who dissented at least in part from the position of the logical positivists, but the positivistic interpretation of scientific inquiry dominated philosophy of science until recently. In the 1950s, however, there developed substantial opposition to their interpretation; and by the mid-1960s a philosopher of science, Dudley Shapere, could describe the developments in his field as follows:

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[12] See Mannheim's essay entitled "On the Interpretation of 'Weltanschauung,'" in *Essays on the Sociology of Knowledge*, pp. 33–83; and Hodges, *Wilhelm Dilthey*, pp. 11–36, 68–87. For a valuable collection of Mannheim's writings that contains a lengthy and penetrating introduction to his thought, see Kurt H. Wolff, ed., *From Karl Mannheim* (New York: Oxford University Press, 1971).

> In the past decade, a revolution—or at least a rebellion—has occurred in the philosophy of science. Views have been advanced which claim to be radically new not only in their doctrines about science and its evolution and structure, but also in their conceptions of the methods appropriate to solving the problems of the philosophy of science, and even as to what those problems themselves are.[13]

As Shapere goes on to indicate, this revolution in the philosophy of science is directed against the view of science associated with logical positivism or logical empiricism. But what alternative epistemological position did the antipositivists adopt? I shall argue that their theory of knowledge is fundamentally historicist. The recent developments in the philosophy of science must be understood in light of the century-long struggle between positivism and historicism. Positivism, having steadily lost ground in the field of philosophy generally, came under siege finally in its last stronghold, the philosophy of science. I shall attempt to establish the historicist nature of the antipositivist revolt in the philosophy of science by examining the two principal sources of influence upon it: (1) developments in philosophy that are associated with the later work of Wittgenstein, and (2) the new interpretation of the history of science.

Ludwig Wittgenstein entered Cambridge in 1912 to study philosophy under Bertrand Russell, who had argued that a new and more rigorous language was needed for discussing the world of nature. Russell envisioned a scientific language whose basic propositions would each denote some object of sense experience, some atomic fact, and whose syntax would conform to the new rules of mathematical logic that he and Whitehead had worked out in *Principia Mathematica*. Wittgenstein developed a position much like Russell's in his *Tractatus Logico-Philosophicus*. This work, which was completed in 1918 and published a few years later in both German and English, sets forth a theory of language that was to influence profoundly the members of the Vienna Circle. Wittgenstein agrees with Russell that the reform of philosophy will require a restructuring of language according to the logic of the *Principia*. This language will be built from elementary propositions, whose signs correspond to objects in the real world. All complex significant propositions will be constructed by logical operations from these elementary propositions and depend on them for their truth or falsity.

Two crucial assumptions underlie Wittgenstein's early position. First, he holds that there is but one ideal language into which philosophical discourse must be cast, but he is unwilling to argue that the logical form of his ideal language has a necessary basis in reason or in nature. Indeed, the ground of language cannot be discussed meaningfully because all language about language is merely tautologous. Second, he holds that an unambiguous correspondence can be established between the unit-propositions of the ideal language and the "atomic facts" that are given in experience. These propositions are said to derive their meaning from the units of experience to which they correspond.

These two assumptions, and with them Wittgenstein's entire position, are obviously vulnerable to attack. If there is no rational or ontological ground for the particular linguistic structure that he advances, then what basis is there for assigning it a privileged or ideal status? The choice of one language rather than another, insofar as it is not simply arbitrary, might arise from convention or agreement. Furthermore, one may doubt that experience as given falls neatly into elementary units which convey an unambiguous meaning. It might be that experience is always open to several possible meanings and that our choice of a language, or a set of concepts, predetermines the particular meaning that we will assign to experience.

By the early 1930s Wittgenstein had come to question his own early views and, indeed, to argue explicitly against them on these very grounds. He gives up the notion that factual language must have a uniform structure, modelled on the language of logic. He moves to the view that there are many alternative languages, or "language games," including, for example, the simple language of a child, the ordinary language of everyday life, and the sign-systems of logic and mathematics. Furthermore, Wittgenstein gives up the theory that language is an exact picture of reality, deriving its meaning from the experience that it depicts. The meaning of words is determined not by private experiences, which in principle are always open to many possible interpretations, but by conventional usage. In effect, he reverses his original view that language receives its meaning from experience and takes the position that experience receives its meaning from language. As many commentators have pointed out, the concepts of language function for the later Wittgenstein in much the same way as the *a priori* categories of the mind function for Kant, that is, they determine the construction of meaningful objects from unformed experience. We know the world only as ordered and articulated by a language; and the only facts or objects belonging to this world are those which our language recognizes.[14]

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[13] "Meaning and Scientific Change," in Robert G. Colodny, ed., *Mind and Cosmos* (Pittsburgh; University of Pittsburgh Press, 1966), pp. 41–42.

Wittgenstein moved in his intellectual development from a position that is favorable to positivism to one that is fundamentally historicist. Language, he came to hold, cannot be regarded as an isolated structure, separate from other expressions of human life and action. Our linguistic activities are part of a total cultural-linguistic tradition, or "form of life," that serves as the basis of society. In his later period Wittgenstein treated the various forms of life which have developed historically as irreducible ultimates, beyond which reasoning and inquiry cannot penetrate: "What has to be accepted, the given, is—so one could say—*forms of life*."[15] Human thought is relative to a language and to concepts which themselves depend on the given form of life. There is no possibility of going beyond or transcending in any meaningful way the various forms of life. As Stanley Rosen has pointed out, Wittgenstein's later position resembles the view of the German historicists that "life" is at the bottom of the *Weltanschauungen* that emerge from the historical process and determine the character of human thought:

> What it means to think, and hence to mean, is decided within the context of an already understood language, more specifically a language in a given historical stage, an all-encompassing horizon of intentions which must be accepted as the basis for analysis or use-specification—a *Lebensform.* The Lebensform is Wittgenstein's equivalent to the nineteenth-century conception of Weltanschauung, a word he uses once to ask whether it is not what "earmarks the form of account we give, the way we look at things." The inaccessibility of the Lebensform to theoretical evaluation replaces the earlier impossibility of speaking about logical form. In the present case, the Lebensform is equivalent to the language or language game, that is, to custom or use. In both stages of his teaching, Wittgenstein denies the possibility of a logos or theoretical account of speech, in both cases because he identifies the horizon of speech as that about which one must remain silent. Formerly, however, one had to remain silent about logical form, or what may be called the natural order of the world. Now one must be silent about the foundations—because there is no natural order of the world. There is no *physis,* so far as philosophy is concerned, but only nomos.[16]

A second major influence shaping the anti-positivist revolution in the philosophy of science has been the new interpretation of the history of science. It seems likely that philosophers of science turned to history for guidance about the nature of science after they came to accept the radical historicity of the human mind and thus to doubt that science has an absolute basis in epistemology or logic. By the early 1930s the Polish logician Kasimir Ajdukiewicz had concluded, on the basis of his historicist theory of language, that science itself must be understood in terms of history.[17] Ajdukiewicz held that all knowledge, including systems of science, embodies concepts that are part of a language and have meaning only when used according to the rules of that language. The commitment to a language and to its conceptual apparatus determines the interpretation that we will place on the data of experience. Consequently, there are a variety of "world-perspectives," corresponding to the various linguistic and conceptual systems, but never a complete "world-picture." Judgments of truth or falsity are always relative to a particular world-perspective. Even the epistemologist is unable to decide between the competing claims of different world-perspectives, for he also is imprisoned in a conceptual apparatus which dictates his judgments. Ajdukiewicz thus recommends that the epistemologist give up his effort to establish criteria for right thinking about the world and turn his attention instead to "the history of science":

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[14] For Wittgenstein's early position, see his *Tractatus Logico-Philosophicus,* trans. C. K. Ogden (London: Kegan Paul, 1922). His later views are developed principally in two posthumous works, *Philosophical Investigations,* trans. G. E. M. Anscombe (New York: Macmillan, 1953) and *Preliminary Studies for the "Philosophical Investigations": Generally Known as the Blue and Brown Books* (New York: Harper, 1958). For studies of Wittgenstein's thought, see, in addition to Urmson's *Philosophical Analysis,* David Pole, *The Later Philosophy of Wittgenstein* (London: Athlone Press, 1958); Ernst K. Specht, *The Foundations of Wittgenstein's Late Philosophy,* trans. D. E. Walford (New York: Barnes and Noble, 1969); and E. D. Klemke, ed., *Essays on Wittgenstein* (Urbana: University of Illinois Press, 1971). It should be noted that the constructivist theory of language is by no means exclusive to Wittgenstein. Similar views were developed at about the same time by Cassirer and by the Polish logician Kasimir Ajdukiewicz, whom I discuss later. Among social scientists, Edward Sapir and Benjamin Lee Whorf developed the influential hypothesis that all views of reality are determined in a largely unconscious manner by the linguistic systems of groups or cultures. See Max Black, "Linguistic Relativity: The Views of Benjamin Lee Whorf," *Philosophical Review,* 68 (April, 1959), 228–238.

[15] Wittgenstein, *Philosophical Investigations,* p. 226.

[16] Rosen, *Nihilism,* p. 9.

[17] See Ajdukiewicz, "The Scientific World-Perspective," in Herbert Feigl and Wilfrid Sellars, eds., *Readings in Philosophical Analysis* (New York: Appleton-Century-Crofts, 1949), pp. 182–188.

> He should give his attention to the changes which occur in the conceptual apparatus of science and in the corresponding world-perspectives, and should seek to ascertain the motives which bring these changes about. Perhaps this sequence of world-perspectives permits of being conceived as a goal-directed process which advances as though someone consciously wished to achieve the goal by means of the sequence. The task involved in such a conception of the history of science constitutes the sound kernel of the *geisteswissenschaftlichen* (culture-theoretical) understanding of the evolution of science.[18]

Contemporary historians of science, proceeding from historicist assumptions that are very close to those of Ajdukiewicz, consider and reject the suggestion that the history of science might be intelligible as a goal-directed process. The basic document for understanding the new interpretation of the history of science is Thomas Kuhn's monograph, *The Structure of Scientific Revolutions*.[19] Kuhn holds that the common textbook image of science has been called into question by recent studies in the history of science; and his purpose is to make explicit the decisive transformation in the image of science that these historical studies require. What Kuhn calls the textbook image of science is, in effect, the one that has traditionally been associated with positivism. It holds that scientific development takes place incrementally or cumulatively within an open community. As knowledge accumulates, it is incorporated into ever more inclusive theories, which are accepted or rejected according to their ability to withstand empirical testing. This process of development-by-accumulation is seen to move steadily toward the ultimate goal of a true and objective account of nature. Kuhn, by contrast, describes the evolution of science in much the same way that the historicist describes the historical process, namely, as a mere sequence of epochs, each with its distinctive worldview, no one of which can claim to represent a closer approximation to the truth about the whole than any other.

In Kuhn's account, a scientific community, during a period of "normal science," is governed by a pervasive "paradigm." Kuhn's definitions of "paradigm" and his descriptions of its influence are somewhat vague and ambiguous, but the similarity of this concept to the older notion of *Weltanschauung* is unmistakable. The paradigm is a scientific community's "historical perspective," its "way of seeing the world," its comprehensive set of beliefs and commitments—conceptual, theoretical, instrumental, methodological, and even "quasi-metaphysical." The paradigm guides and determines the selection of problems, the evaluation of data, and the acceptance of hypotheses and theories. By taking first principles for granted in this manner, "normal scientists" are able to explore a small range of relatively esoteric problems in great depth and detail, but the price, Kuhn emphasizes, is a high degree of rigidity and narrowness within the scientific community itself. Young scientists are trained to accept the paradigm uncritically by textbooks that ignore or depreciate alternative outlooks. Problems are avoided, even socially relevant ones, if they cannot be stated in terms of the conceptual and instrumental tools that the paradigm supplies. Discoveries or inventions that would call the paradigm into question are suppressed. Scientists with dissenting views are read out of the profession and their work is ignored.

By contrast to the positivistic understanding of scientific development, which stresses incremental or cumulative progress, Kuhn describes the historical pattern of scientific development as a noncumulative shift from one paradigm to another. He emphasizes that a drastic change in the character of scientific inquiry comes about when one paradigm or set of presuppositions is abandoned and another takes its place. Scientists follow new criteria in selecting problems, interpreting data, and assessing theories. The old criteria and the body of knowledge established through them are, in effect, given up rather than being incorporated into the new stage.

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[18] Ajdukiewicz, p. 188.

[19] Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970). Dudley Shapere has examined this work and called attention to its relativistic implications in *Philosophical Review*, 73 (July, 1964), 383–394. Kuhn replies to Shapere and other critics in a "Postscript" to the second edition of his work. He reaffirms that a theory cannot be true in the sense of accounting for nature or reality as it is (p. 206).

Kuhn's historicism comes clearly into view in his attempt to explain why these "scientific revolutions" occur. If there is to be a scientific revolution, scientists must become aware of some anomalous fact that the present paradigm cannot explain. In this time of crisis for normal science, new theories are invented to account for the anomaly, one of which eventually gains wide acceptance as the core of a new paradigm. It would be a mistake, however, to say that theories or paradigms are accepted or rejected in terms of their congruence with empirical data. To those who hold that scientific theories are rejected when they are falsified or contradicted by experience, Kuhn replies that the paradigm shapes observation itself, reducing the likelihood that scientists will even perceive data that are contrary to expectations that the paradigm itself engenders. Also, facts are always present that do not "fit" the accepted theory, but these are seldom regarded as serious anomalies during periods of normal science. To those who hold that new theories are accepted when they are confirmed by experience, Kuhn replies that it is always possible to place more than one theoretical construction upon a given collection of data. Clearly there are forces at work in scientific revolutions that are more fundamental than the confrontation of the scientist with the data of experience. Kuhn compares paradigm shifts to the perceptual transformations that occur when one looks at the ambiguous pictures used in gestalt experiments. The perceptual data themselves are not decisive in the mind's interpretation of the data. It would be a mistake also to believe that paradigms are accepted or rejected on rational or logical grounds. Inasmuch as paradigms are "incommensurable," they cannot be judged by common criteria. In short, a change in paradigms cannot be explained as a deliberate choice, based on either empirical or rational grounds. In the final analysis, "[a]n apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time."[20] What is arbitrary is, of course, inexplicable.

Let us recall Ajdukiewicz's suggestion that the history of science might be intelligible as a goal-directed process. Ajdukiewicz had obviously been reluctant to break completely with the positivistic belief in the progressive character of scientific development. Kuhn admits that within a period of normal science there can be progress, judging by the criteria of the prevailing paradigm, toward the solution of technical problems or puzzles. He doubts, however, that the succession of changes from one paradigm to another has brought scientists closer to the truth about nature. He asks: "Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal?"[21] Kuhn doubts that this question can be answered affirmatively; and the reason for his doubts is clear. Just as the older historicists could admit no standpoint outside of a particular worldview for grasping the truth about nature or history, Kuhn can admit no standpoint outside a particular paradigm. There is no trans-paradigmatic basis for assessing the truth of the competing paradigms or for apprehending nature as it is. It is no wonder that he tells us that the decision for a particular paradigm "can only be made on faith."[22]

Having examined the sources of the contemporary revolt against positivism in the philosophy of science, let us look briefly at the position of those philosophers who are identified with this revolt. Among the leaders are Stephen Toulmin, Michael Scriven, Norwood R. Hanson, Paul Feyerabend, and, in social science, Peter Winch. The grounds of their opposition to the positivist view of science have been treated in detail by Dudley Shapere, who, while critical of their work, nonetheless recognizes its fundamental importance.[23] It is clear from Shapere's account that the antipositivist position has much in common with the philosophical views of the later Wittgenstein and with views of the history of science such as Kuhn's. Their overall interpretation of science is fundamentally historicist, or, as Shapere prefers to say, relativistic. Shapere identifies as the chief substantive characteristic of the antipositivist reaction "the view that, fundamental to scientific investigation and development, there are certain very pervasive sorts of presuppositions" which vary from one scientific theory or tradition to another. We are reminded of the view that all thought and experience are determined by a *Weltanschauung* (Dilthey, Mannheim) or by the customary language and *Lebensform* (Wittgenstein) or by a paradigm (Kuhn). Shapere identifies three distinctive features of the new philosophy of science that follow from this principle. First is "a presupposition theory of meaning," according to which the meanings of all scientific terms, whether factual or theoretical, are determined by the underlying paradigm or theory or set of presuppositions. This theory of meaning contradicts the positivist thesis that the meaning of scientific terms is derived directly or indirectly from experience and, indeed, that experience is the absolute ground by which the meaningfulness of any statement about the world must be judged. This presupposition theory of meaning is based, of course, on the assumption that presuppositions or theories will inevitably shape perception itself and determine what will count as a fact. A second feature of antipositivist philosophy of science is " a presupposition theory of problems that will define the domain of scientific inquiry, and of what can count as an explanation in answer to those problems." By holding that the very character of explanation varies from one context to another, the antipositivists take issue with the positivist view that all scientific explanation must conform to the same formal model, namely, the "deductive-nomological" model, according to which there can be no explanation without a general law. In the third place, the antipositivists adopt "a presupposition theory of the relevance of facts to theory, of the degree of relevance (i.e., of the relative importance of different facts), and, generally, of the relative acceptability or unacceptability of different scientific conclusions (laws, theories, predictions)." This thesis contradicts the positivist view that all scientific theory must be tested against experience according to some standard principle of verifiability, confirmability, or falsifiability.

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[20] Kuhn, *The Structure of Scientific Revolutions*, p. 4.

[21] Kuhn, p. 171.

[22] Kuhn, p. 158.

[23] Shapere, "Meaning and Scientific Change," pp. 41–85. In this essay, Shapere focuses particularly on the writings of Paul Feyerabend, which are cited extensively, along with pertinent writings by other anti-positivists, in the footnotes. For other accounts of this cleavage, along with useful bibliographical information, see Israel Scheffler, *Science and Subjectivity* (Indianapolis: Bobbs-Merrill, 1967); Achinstein and Barker, eds., *The Legacy of Logical Positivism;* Peter Achinstein, *Concepts of Science* (Baltimore: Johns Hopkins Press, 1968); Edward MacKinnon, S. J., "Epistemological Problems in the Philosophy of Science, I, II," *Review of Metaphysics*, 22 (September and December, 1968), 113–137, 329–358; and Carl R. Kordig, "The Theory-Ladenness of Observation," *Review of Metaphysics*, 24 (March, 1971), 448–484. There are several excellent series in which one may survey recent debate over the nature of science, including *Boston Studies in the Philosophy of Science, University of Pittsburgh Series in the Philosophy of Science,* and *Minnesota Studies in the Philosophy of Science*. The most recent volume of the Minnesota Studies, *Analyses of Theories and Methods of Physics and Psychology*, ed. Michael Radner and Stephen Winokur (Minneapolis: University of Minnesota Press, 1970), contains several articles on theory that take note of recent controversy.

We see that the antipositivist movement in the philosophy of science has reinterpreted modern science, including natural science, in light of the characteristic assumptions of historicism. It suggests that the guiding presuppositions of scientific inquiry are variable from one context to another and are ultimately arbitrary inasmuch as they have no absolute basis in reason or experience. Even some opponents of this movement have come to agree with certain of its basic assumptions, for example, the assumption that modern science rests on arbitrary presuppositions.[24] Yet if modern science rests on presuppositions that are finally arbitrary, one must wonder if there is any basis whatever for defending it as superior in principle to the modes of inquiry that its proponents have, from the outset, repudiated.

The Growth of Historicism in American Political Science

Thus far I have given attention to the emergence of historicism in Germany, its conflict with positivism over the character of human knowledge, and its influence in one field of philosophy—the philosophy of science—after 1930. If space permitted, it would be fruitful to trace the development of similar theories of knowledge in other countries and to consider the influence of this general epistemological position on other fields of philosophy and other philosophical movements.

Our task now is to examine the recent flowering of historicism in American political science. While traditional currents of American thought, such as pragmatism, have no doubt contributed to this development, there are four sources of historicist influence which seem particularly important and whose impact I wish to consider: (1) antipositivist writings in the philosophy and history of science; (2) writings in the sociology of knowledge that adopt the position of Karl Mannheim; (3) recent work in existential phenomenology; and (4) the thought of Friedrich Nietzsche. Of the political scientists who draw from these sources, some freely embrace a thoroughgoing relativism, while others avoid or resist the conclusion that genuine or final knowledge is impossible. I shall attempt to show that relativism becomes unavoidable once certain premises of historicism are granted.

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[24] For example, Ernest Nagel has conceded that "the principle of causality" (his term for what others have called the principle of the uniformity of nature) must be regarded as an indispensable or logically necessary maxim by anyone who chooses to adopt the goals of explanation and control as conceived by modern science. Yet he admits that the choice between modern theoretical science and other views of knowledge is itself historically contingent and logically arbitrary. In other words, there seems to be no basis in reason, nature, or history for regarding the underlying assumptions of modern science as more true or valid than those that underlie any other type of explanation that might be chosen by men of another epoch. See *The Structure of Science*, pp. 316–324.

In justifying their particular conception of science, advocates of a behavioral approach have commonly invoked the authority of the philosophy of science. They have assumed—and a generation of political scientists has been taught to believe—that there exists within the philosophy of science a consensus favoring a positivistic conception of scientific inquiry. We have seen, however, that there is a basic conflict about the nature of science within the philosophy of science itself, arising from the broader dispute at the level of epistemology about the character of human knowledge. Ironically, a revolt against positivism was beginning in the philosophy of science at about the time that political scientists were adopting the positivistic model of scientific inquiry as the basis for their own revolution. By the time the behavioral revolution had reached its objective, the shape of things in the philosophy of science had changed dramatically. Nevertheless, the methodological literature in political science continues to treat the positivistic model as though it enjoyed the full endorsement of philosophers of science. The writing of such men as Kuhn and Hanson are, in fact, often cited in support of the behavioralists' program, with little apparent recognition of the extent to which their views jeopardize the very foundations of behavioralism.

One political scientist who has drawn attention to the lag or gulf between contemporary work in the philosophy of science and political scientists' perceptions of that field is John Gunnell. Agreeing generally with the epistemological position of such antipositivists as Kuhn, Hanson, Toulmin, and Feyerabend, Gunnell questions several features of the positivist conception of science, particularly its deductive model of explanation. The deductive model, he charges, is not an accurate representation of the actual logic of inquiry in natural science. And even if this logic were accurate insofar as natural science is concerned, there would be no basis for insisting that social science should conform to it. The canons of scientific inquiry for a particular field must be determined by scientists who work in that field and not by some outside authority.[25] This is not to say that Gunnell is without convictions of his own regarding the proper approach to political inquiry. He would replace the behavioral approach with one that draws from the insights of Max Weber, the later Wittgenstein, and phenomenologists such as Alfred Schutz. In brief, Gunnell argues that each society organizes itself around certain symbolic forms, which determine its view of social reality and define for its members the possibilities of action. The meaning of human action is to be found in the symbolic context from which it arises. The task of the social scientist is to illuminate the symbolic contexts that give meaning to action in various societies. To explain action is to give its meaning for the actor by reference to symbols and concepts that are operative in the social situation.[26]

Gunnell wishes to throw off the burden that a slavish adherence to the positivist model has imposed on political inquiry, but one must wonder if that effort can succeed on the grounds on which he has chosen to base it. He could reach his objective by arguing, on philosophical grounds, that the human things are essentially different from the nonhuman things and must be understood by methods of inquiry that are different from those now employed in natural science. In fact, he seems to take this very path in defending his own view of explanation against that of logical empiricism, for he holds that social science must deal not with a "first order reality," like the natural world of concern to physical science, but with a "second order reality," namely, "a world of intersubjectively shared symbols, a logically ordered world that has been pre-constituted by the participants and in whose terms action is conducted and justified."[27] Here Gunnell seems to argue that a variety of methods is required by the nature of things and the nature of understanding. In another context, however, he attempts to protect social science against the doctrinaire claims of a particular school of philosophy by rejecting philosophy altogether as the measure of empirical science. He adopts the view of Kuhn and others that the meaning of science for a particular field is determined entirely by convention, or by the existing "paradigm":

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[25] Gunnell, "Deduction, Explanation, and Social Scientific Inquiry," *American Political Science Review*, 63 (December, 1969), 1233–1246. The same issue contains replies to Gunnell by Arthur S. Goldberg and A. James Gregor, along with a rejoinder by Gunnell, pp. 1247–1262. Gunnell draws upon antipositivist accounts of theory and observation to oppose the prevailing epistemological assumptions of political scientists in "The Idea of the Conceptual Framework: A Philosophical Critique," *Journal of Comparative Administration*, 1 (August, 1969), 140–176. Thomas Landon Thorson's *Biopolitics* (New York: Holt, Rinehart and Winston, 1970) is another work by a political scientist that discusses sympathetically the antipositivist revolt in the philosophy of science.

[26] Gunnell, "Social Science and Political Reality: The Problem of Explanation," *Social Research*, 35 (Spring, 1968), 159–201.

[27] Gunnell, in *Social Research*, p. 180.

> The meaning of "science" at any time or place is determined by the paradigm which informs scientific activity and reasoning and which specifies the conception of reality and the phenomena to be investigated, designates problems for investigation, and determines the criteria of acceptable explanation and inference.[28]

Philosophy can analyze and clarify the language of science and explore problems that science raises, but it cannot provide an authoritative critique of the "framework of ideas about reality, theories, and rules which together constitute a paradigm or conceptual scheme for interpreting and organizing experience."[29] Gunnell denies that this contextualist approach abandons the ideals of objectivity and intersubjectivity, but he admits that objective or intersubjective standards in science are always relative to a particular context or paradigm. Neither logic nor epistemology can establish "transcontextual standards" for science.

By holding that the meaning of science at any time or place is relative to the prevailing paradigm, Gunnell may win his battle against those outside of social science who would impose on it a reconstruction of scientific method based on natural science, but he has left social science at the mercy of whatever paradigm should arise there from fate or choice. Let us suppose that contrary to Gunnell's wishes, the method of natural science comes to be accepted as the *de facto* paradigm in the social sciences. His contextualist position leaves us with no philosophical basis for questioning its authority. In order to be strictly consistent, Gunnell would have to concede that his own preferred methodology is but a recommendation to the community of social scientists about a possible consensus or paradigm for inquiry. He has undermined any possible claim that it is founded upon a transcontextual grasp of the nature of things or of reality. The basis for Gunnell's position is not so much a belief in the autonomy of scientific inquiry as a scepticism about the capacity of philosophy itself to move from opinions, or conventional representations of reality, to knowledge of the nature of things. Philosophical speculation, like scientific, religious, and common-sense thinking, must take place within its own distinctive paradigm or conceptual framework which, we may suppose, is ultimately conventional or historical. Gunnell might avoid the relativism that is implicit in the position of Kuhn and others and find a secure foundation for his own methodology by granting that the mind can transcend all paradigms or presuppositions and test them by its grasp of the permanent structure of reality. He holds instead that all thought and observation are relative to some particular community's conceptualization of the real world.[30]

Sheldon Wolin has considered both behavioral and traditional political theory in light of the new interpretation of science. In "Paradigms and Political Theories,"[31] Wolin develops Kuhn's position at length, drawing attention, we must note, to the arbitrary element that is involved in the choice of scientific paradigms. He shows quite effectively that Kuhn's interpretation undercuts the assumptions about the nature and development of science that have guided the behavioral movement in political science. He goes on to argue that the history of political theory can be understood in terms of Kuhn's model of scientific development. In particular, he compares the role of the "epic theories" in the history of political thought to that of paradigms in the history of science. In his view such epic theorists as Plato, Aristotle, Machiavelli, Hobbes, Locke, and Marx "considered theorizing to be an activity aimed at the creation of new paradigms."[32] These "paradigm-creators" established new visions of the political world which were then applied and developed by less original theorists, or "paradigm workers," who correspond to the "normal scientists" of Kuhn's analysis. Wolin argues further that political societies may also be understood as embodiments of paradigms of an operative kind, namely, the totality of institutions, practices, and beliefs in accordance with which the society carries on its political life. Two important points are made about the relationship of theoretical and operative paradigms. First, the epic political theorist, unlike the extraordinary theorist in natural science, seeks to change the world about which he speculates (i.e., the social world) and not merely the way men look at the world. He may accomplish this by persuading rulers to adopt his theory as the social paradigm or by winning acceptance for it in the universities. Second, the major political theories are a response not to a theoretical crisis but to a political crisis, to a condition in which the operative paradigm of society cannot adapt to challenges or changes. The theory seeks to discredit and to replace the operative paradigm. Wolin closes with a suggestion about the nature of the paradigm that guides behavioral studies in political science: Its assumptions and criteria are derived not from a general theory but from the operative paradigm of the liberal or democratic regime in which it is conducted.

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[28] Gunnell, "Deduction, Explanation, and Social Science Inquiry," p. 1244.

[29] Gunnell, "Deduction, Explanation . . .," pp. 1245–46.

[30] Gunnell's position may be contrasted with the more cautious position of Berger and Luckmann, to which I refer later. He does not restrict himself to the methodological assertion that the social scientist must grasp a society's understanding of social reality in order to explain action within that society. He seems to embrace the philosophical or epistemological view that all definitions of social reality are relative to some community and, furthermore, that one can never move beyond these various definitions to a true understanding of the nature of society as such. See particularly Gunnell's "Social Science and Political Reality," pp. 184–186. Neither reason nor experience offers a standpoint on which an objective understanding of social reality can be based. The attempt to construct an independent definition of social reality by reasoning must employ arbitrary or contingent assumptions (pp. 160–68). Moreover, an objective account of social reality cannot be built on experience because there are no independent or autonomous observations. (pp. 178–80).

[31] In *Politics and Experience: Essays Presented to Michael Oakeshott*, ed. Preston King and B. C. Parekh (Cambridge: Cambridge University Press, 1968), pp. 125–152.

[32] Wolin, in *Politics and Experience*, pp. 139–140.

In a subsequent essay, "Political Theory as a Vocation,"[33] Wolin develops much the same position, but expands his analysis and critique of behavioralism. Specifically, he argues that a belief in the primacy of method, resembling the methodological emphasis of Descartes, is the distinguishing feature of the behavioral movement. In the absence of a general theory, the assumptions about man and society that guide behavioral research have been drawn from the prevailing political ideology. Consequently, the goodness or normality of the established system is presupposed in research and theorizing alike. Empirical theories may provide explanations of why things happen as they do, but they can offer "no significant choice or critical analysis of the quality, direction, or fate of public life."[34] Wolin contends that it is the vocation of the political theorist to offer a critique of the established order and a vision of new possibilities. He criticizes contemporary education in political science for depreciating that "tacit political knowledge" which is a precondition of critical theory.

Wolin's analysis provides another illustration of the manner in which historicist arguments originating in the philosophy of science can be used against behavioral political science. Yet as in Gunnell's case, there is no evidence that Wolin can avoid the relativistic implications of these arguments for political theory itself. The central problem to emerge from Wolin's discussion is the epistemological status of the "epic political theories." This problem is not dealt with directly, but a solution is implied. Wolin would certainly deny that political theory is merely a reflection of the prevailing worldview of society. By definition, theory must be critical of established opinions and institutions. In Wolin's account, theory is more a source than a consequence of society's operative paradigm. In the final analysis, however, theories seem to rest on presuppositions whose truth or congruence with nature cannot be established conclusively by reason or experience. Let us recall that Wolin likens an epic political theory to a scientific paradigm in Kuhn's sense of the term. He emphasizes the fact that for Kuhn, there is no objective way of deciding among competing paradigms. An "arbitrary element" ultimately guides the selection. It is a commitment that must rest on faith, not on a cognitive grasp of nature or reality as it is. Now, Wolin points to several differences between political theories and scientific paradigms, but he never denies that they are alike in the ultimate arbitrariness of their fundamental assumptions. It is not merely Wolin's silence here that leads one to believe that political theories are finally arbitrary. We may note that he speaks consistently of "choosing" or "creating" a theory rather than of developing or discovering theoretical knowledge. Granting that the theorist can be "highly self-conscious" about choosing among theoretical alternatives,[35] does nature provide a standard for guiding the choice? Wolin adopts the argument of Kuhn, Hanson, and others to the effect that we perceive "facts" only through concepts that are usually derived from a theory. Creativity and imagination must play a large part in theorizing because facts are not univocal. But is "nature" also relative to a theoretical commitment, or is it there prior to theorizing to be discovered as it is? It seems doubtful that Wolin would concede this, even though he claims to be restating the older understanding of theory, of the *bios theoretikos*. One suspects that Wolin has liberated theory not merely from bondage to empirical fact but from any obligation to represent nature or reality as it is.

A solution to our problem seems to depend on Wolin's understanding of what he, following Michael Polanyi, calls "tacit knowledge." Wolin contends that political theory is built on tacit political knowledge. More specifically, the source of theoretical vision and creativity is "the stock of ideas which an intellectually curious and broadly educated person accumulates and which come to govern his intuitions, feelings, and perceptions." But are these "ideas" themselves the result of vision, of the mind's grasp of the nature of things? Wolin speaks of them as "human creations," not as discoveries. As presuppositions of thought, they rarely find theoretical expression. They "bear a family resemblance to 'bias.' " It would appear that they are historically conditioned and, with respect to their rational ground, arbitrary.[36]

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[33] Wolin, "Political Theory as a Vocation," *American Political Science Review*, 63 (December, 1969), 1062–1082.

[34] Wolin, "Political Theory as a Vocation," p. 1063.

[35] Wolin, "Political Theory," p. 1075.

Turning now to our second source, we must note that Karl Mannheim's writings have been less influential in political science than in sociology, where the sociology of knowledge has been an established field for more than three decades. He is perhaps best known among political scientists for his discussion of the concept of ideology. It is important to recognize that Mannheim's treatment of ideology is deeply influenced by his own commitment to historicism. His "general-total" conception of ideology, "according to which the thought of all parties in all epochs is of an ideological character," merely restates his own historicist thesis that all thought, including that of philosophers and social scientists, is socially determined and historically variable.[37] Most political scientists who acknowledge a debt to Mannheim in their own discussions of ideology have nevertheless defended the ideal of scientific knowledge against Mannheim's relativistic conclusions. It is questionable, however, whether one can reasonably uphold the ideal of scientific objectivity after having accepted the bulk of Mannheim's argument for the ideological (or historicist) character of human thought.

The difficulties inherent in this undertaking come to light in William E. Connolly's *Political Science and Ideology*.[38] Connolly agrees with Mannheim that all thinking, including scientific inquiry, gives expression to a particular *Weltanschauung* or "perspective," which is the product of the thinker's epoch, culture, and social position. Mannheim was correct in holding that mental productions are linked to the social process, although he failed to give an adequate explanation of this link—a defect which Connolly remedies by drawing upon George Herbert Mead's historicist account of language. The perspective supplies an individual with his fundamental beliefs or presuppositions, his values, and the concepts that he uses in thinking, speaking, and interpreting his experience. In order to engage in political activity in situations of relative uncertainty, men develop ideologies, i.e., systems of beliefs or empirical claims which purport to describe and explain the social and political environment. An ideology grows out of a perspective. It construes the environment in ways that are congruent with and supportive of that perspective. It contains beliefs and assumptions about the world that have not been fully tested, but which are accepted, as it were, on faith. It resists falsification by assimilating information in ways that protect these assumptions, as well as the individual's higher level beliefs and values.

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[36] Wolin, "Political Theory," pp. 1073–1074. Wolin refers us to Michael Polanyi, but Polanyi's stand regarding the problem of historicism is not altogether clear. Marjorie Grene, in *The Knower and the Known* (London: Faber and Faber, 1966), emphasizes the similarity of Polanyi's approach to the *Lebensphilosophie* of Dilthey and the existential phenomenology of Merleau-Ponty. On the other hand, Helmut Kuhn argues that Polanyi does nothing less than overcome the "crisis of the philosophical tradition generally known under the title of 'historicism.' " According to Kuhn, Polanyi regards genuine knowledge not as a creation of the mind but as the mind's discovery of the character of reality through contact with it. See Kuhn's "Personal Knowledge and the Crisis of the Philosophical Tradition," in *Intellect and Hope: Essays in the Thought of Michael Polanyi*, ed. Thomas A. Langford and William H. Poteat (Durham: Duke University Press, 1968), pp. 111–135. Wolin does not indicate which of these interpretations he would prefer.

Other writings of Wolin seem also to take a position that is implicitly relativistic. In *Politics and Vision* (Boston: Little, Brown, 1960), pp. 17–21, he distinguishes between two meanings of vision, i.e., "objective" vision, which seeks to describe an object or event dispassionately, and "imaginative" vision, which constructs models that reflect the theorist's own fundamental values. Imaginative vision, it would seem, is always from a "perspective" and cannot claim to grasp reality as it is. The perspectival character of theory is emphasized also in "Political Theory: Trends and Goals," *International Encyclopedia of the Social Sciences*, ed. David L. Sills (New York: Macmillan and The Free Press, 1968), XII, 318–331, where Wolin asks: "Does each theory present us with a different political world? Is political theory a bedlam of subjectivity and relativism? How does one decide whether one theory is truer than another? Is the history of political theory merely a succession of different theories, instead of successive additions to our knowledge and understanding of politics?" Having raised the crucial issues, he replies that "[t]hese questions cannot be answered here, even assuming that they can be answered satisfactorily at all" (pp. 322–323). His statements about the nature of facts and concepts seem, however, to rule out the very possibility of a definitive understanding of nature or reality.

[37] *Ideology and Utopia*, trans. Louis Wirth and Edward Shils (New York: Harcourt Brace, 1949), pp. 55–108. Mannheim's position is related to broader currents of modern thought in Maurice Mandelbaum, *The Problem of Historical Knowledge* and H. Stuart Hughes, *Consciousness and Society* (New York: Knopf, 1958). For Mannheim's influence on contemporary sociology, see Albert Salomon, "Karl Mannheim, 1893–1947," *Social Research*, 14 (September, 1947), 350–364; Kurt Wolff, "The Sociology of Knowledge and Sociological Theory," in Llewellyn Gross, ed., *Symposium on Sociological Theory* (Evanston, Ill.: Row, Peterson and Co., 1959), 567–602; Kurt Wolff, *From Karl Mannheim;* and Irving Louis Horowitz, *Philosophy, Science and the Sociology of Knowledge* (Springfield, Ill.: Thomas, 1961).

[38] Connolly, *Political Science and Ideology* (New York, Atherton Press, 1967).

Connolly refuses to agree with those who hold that political inquiry is inherently ideological or subjective. He accepts the scientific ideal of political inquiry, the goal of which is to replace ideology by scientific theory, whose assumptions and concepts are defined explicitly and precisely and whose propositions about the world have withstood empirical testing. Yet given the perspectival character of human thought, is there reason to believe that this goal can be achieved? Connolly admits that we can never raise all of our presuppositions and concepts to the level of consciousness. He concedes that we experience the world and interpret our experience in conformity with our broader perspective. He seems to grant that the scientific ideal is unattainable *in practice*. He does not believe, however, that his epistemological premises undercut the scientific ideal *in principle*—a conclusion that is certainly open to question. Let us concede that the scientific ideal is valid in principle, which, for Connolly, means that no one has established conclusively that it cannot be attained. What reason is there to pursue a goal that seems for all practical purposes to be unattainable? One might argue that the goal of modern science, though unattainable in practice, represents the perfection of the human understanding, the highest and most natural attainment of the mind. The great thinkers who laid the epistemological foundations of modern science took this position. Connolly is not willing to claim, however, that the logic of modern science, or its method, or its criteria of validation, are inherently sounder or more natural than those belonging to any other mode of thought. He accepts Mannheim's crucial premise that each perspective has its own view of the aims of inquiry and its own canons of validity. The logic of modern science is but one of these "social emergents." It is not possible to show, on ontological, psychological, or empirical grounds, that this logic is more valid than another. For all we know, the ideal of scientific objectivity cannot be realized in practice nor shown in principle to be sounder than other conceptions of human knowledge. Yet even though this ideal might be impracticable and baseless, Connolly would nevertheless have us strive to attain it; for while this striving might not make our ideologies more true, it will make them more "responsible." In effect, he shifts from epistemological to ethical grounds in attempting to justify the scientific ideal. If Connolly's view of knowledge is correct, however, one must wonder if his criteria of responsibility are not themselves the product of a particular perspective and, in the final analysis, arbitrary.

Whereas *phenomenology* has served as a foundation for psychological and sociological inquiry for decades, especially in Europe, it did not begin to have a noticeable impact on American political science until the mid-1960s. In assessing this impact, one must keep in mind that a commitment to phenomenology as a method of inquiry or even as a philosophical position does not necessarily entail a commitment to an historicist epistemology. As we have seen, the phenomenological movement was, at the outset, strongly opposed to historicism. Although phenomenology and historicism tended subsequently to merge, there is no reason, in principle, that a phenomenologist today might not adopt epistemological principles that favor the search for transcendent and permanent knowledge. Again, social scientists who follow a phenomenological approach might choose, with Peter L. Berger and Thomas Luckmann, to "bracket" epistemological problems, i.e., to leave them to philosophers.[39] Yet as a recent essay by Marvin Surkin shows, a phenomenological approach to political science can be combined with historicism in a radical form.[40]

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[39] See Berger and Luckmann, *The Social Construction of Reality* (Garden City: Doubleday, 1966), pp. 1–17. Berger and Luckmann concede that the philosopher, as opposed to the sociologist, is obligated "to obtain maximal clarity as to the ultimate status of what the man in the street believes to be 'reality' and 'knowledge.' Put differently, the philosopher is driven to decide where the quotation marks are in order and where they may safely be omitted, that is, to differentiate between valid and invalid assertions about the world" (p. 2). One must wonder, however, why social scientists should not attempt to understand opinions about social reality in light of what is really true of society.

[40] See Surkin's "Sense and Non-sense in Politics," in *An End to Political Science*, ed. Marvin Surkin and Alan Wolfe (New York: Basic Books, 1970), pp. 13–33. This essay is a slight modification of one that appeared earlier in *PS*, 2 (Fall, 1969), 573–581. Surkin here outlines a methodology for political science based on existential phenomenology and indicts the behavioral approach on grounds similar to those of Wolin. Despite its claim to objectivity and value neutrality, behavioralism is said to reflect the prevailing ideology and to serve the dominant institutions of

Of those who have criticized the behavioral approach from a phenomenological standpoint, Hwa Yol Jung provides us with the most detailed statements on the implications of existential phenomenology for political inquiry.[41] Jung describes existential phenomenology as the outgrowth of efforts by such thinkers as Heidegger, Sartre, Merleau-Ponty, Paul Ricoeur, and John Wild to synthesize and go beyond the existential thought initiated by Kierkegaard and the phenomenological thought initiated by Husserl. The method of existential phenomenology is essentially descriptive. It seeks to elucidate the reality of man's "being in the world" by describing rigorously the meaning of his concrete experience in what Husserl came to call the *Lebenswelt* (life-world). The life-world is that primordial and familiar world of everyday existence which we take for granted as the archetype of reality.

Three characteristics of the life-world are regarded by Jung as especially important for political inquiry. First, the life-world has meaningful patterns and structures which we apprehend prior to reflection through immediate experience or feeling. It is a world not of "what 'I think' but of what 'I live through.' " This world of "lived experience" is a preconceptual or pretheoretical world. It is "pregiven" to the theorist, i.e., presupposed as the foundation of any explicit act of conceptualization in philosophy or science. Second, the life-world is the theater of action. We participate in it primarily as actors or doers rather than as thinkers. Our active participation in the life-world is to be understood in a dual sense. On the one hand, the life-world serves as the indispensable basis of human action. It is permeated by values, meanings, concerns, and interests that enter into our motivational field and help to determine what we do. On the other hand, the reality of everyday existence is itself created or constituted by action or work. The meaningful objects of consciousness come to view in accordance with the underlying aims or projects of human action. The life-world is a field of *pragmata.* Things in nature as well as cultural artifacts are defined in terms of their serviceability, usability and manipulability. In sum, action involves both a response to the life-world as given and a projection beyond it toward the creation of new meaning or reality. Finally, the life-world is an intersubjective or socialized world. It "refers to the complex living relationships of man to man in culture, in society, in history, and in politics." In everyday life the existence of other selves is never in doubt, for we experience them immediately. Cultural artifacts, especially language, attest to the social character of everyday existence; and our actions are always structured with a view to the reactions and responses of others.[42]

Jung's critique of behavioralism rests on his contention that theoretical knowledge should be founded upon and consonant with our immediate experience of the life-world. For example, a proper definition of a political concept such as "democracy" requires us to turn directly to our "felt or tacit meaning" of democracy. Jung finds, however, that the behavioral approach characteristically formulates its concepts and constructs its models in abstraction from the totality of lived experience. In its eagerness to predict the course of behavior, behavioralism fails to describe political phenomena accurately or to elucidate their meaning. Jung's comparison of behavioralism and phenomenology centers primarily around their respective approaches to the study of human action. The distinctiveness of the phenomenologi-

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American society. Surkin does not claim that his alternative methodology is any less evaluative, ideological, or contingent than behavioral methodology, for in his view, all claims to truth and all modes of philosophical and social inquiry are contingent and socially determined. His methodology is fundamentally different from behavioralism in this respect: recognizing its own contingent and ideological character, it dedicates itself to criticizing and changing the existing society rather than to preserving it.

[41] See especially Hwa Yol Jung, "The Political Relevance of Existential Phenomenology," *Review of Politics,* 33 (October, 1971), 538–563. A revised version of this essay appears as the Introduction to *Existence, Sociality and Political Reality: A Reader in Existential Phenomenology* (Chicago: Regnery, 1972). My analysis of Jung's thought would have been impossible were it not for his generosity in providing me with a copy of the manuscript of "The Political Relevance of Existential Phenomenology," which appeared in print only after the completion of my essay. I have also made use of a revised version of Jung's "A Phenomenological Critique of the Behavioral Persuasion in Politics: A Philosophical View," which was delivered at the 1971 Annual Meeting of the American Political Science Association. Other writings by Jung on existential phenomenology and politics include: "The Radical Humanization of Politics: Maurice Merleau-Ponty's Philosophy of Politics," *Archiv Für Rechts- und Sozialphilosophie,* 53 (1967), 233–256; "Leo Strauss's Conception of Political Philosophy: A Critique," *Review of Politics,* 29 (October, 1967), 492–517; and "Confucianism and Existentialism: Intersubjectivity as the Way of Man," *Philosophy and Phenomenological Research,* 30 (December, 1969), 186–202. For a general account of the influence of phenomenology on social science, see Fred R. Dallmayr, "Existential Phenomenology and Social Science: An Overview and Appraisal," which will appear in 1972 in a collection of essays on phenomenology to be edited by Edward Casey and David Carr and to be published by the Quadrangle Press.

[42] Jung, "The Political Relevance of Existential Phenomenology," pp. 540–543.

cal approach is said to lie in its concern for the meaning of human action, that is, the intended meaning which the actor imputes to his action. Action always embodies a preconceived project, a practical determination of what is to be done. Behind the project is a motive, which consists of a reason for doing something as well as a force that initiates bodily movement. It is especially in the reason for an action that we find its meaning. Behavioralism is unable to grasp the meaning of action because of its commitment to study human phenomena with the methods of the natural sciences. It fails to recognize that these methods, while appropriate for the investigation of merely natural things, are unsuited for the study of human beings, who are capable of endowing their actions and perceptions with meaning. In order to adhere to its canons of scientific methodology, behavioralism must restrict itself to the study of overt behavior and, to the extent that it is concerned with intentions, beliefs, motives, feelings, or values, attempt to infer them from the external indications of action. Without denying the significance of what is observable from the outside, "phenomenology attempts to focus on and capture the meaning of action as the actor lives through it. . . ."[43] A phenomenological approach in political science would understand political events in light of their meanings as interpreted by political actors themselves. This intended meaning is accessible through our experience of the life-world, which is a structure of shared meanings. Our immersion in the life-world allows us to understand the meaning of human action as participants and not simply as observers.[44]

Jung is quite aware that existential phenomenology has been identified by some of its critics as a type of historicism. He denies that this philosophical standpoint leads to an historical relativism,[45] but there is reason to doubt that this outcome can be avoided. We have taken note of an emphasis within the empiricist and positivist traditions on the mind's immediate consciousness of pure, unconceptualized experience that is intrinsically meaningful and that can serve as an absolute ground of general knowledge. While this position is called to mind by Jung's insistence that conceptual or theoretical knowledge must rest upon and be judged by preconceptual experience, he in fact departs from it radically both by opposing the reduction of experience to mere "sense data" and by holding that meanings are actively constituted by consciousness.

In considering Jung's stand with regard to the source of meaning, one finds it helpful to know that there has been disagreement within existential phenomenology on the relationship of consciousness to the world. Some writings, such as Heidegger's *Being and Time* and Sartre's *Being and Nothingness,* indicate that the individual consciousness constitutes its world and endows it with meaning. A meaningful world arises from the willful projects of the individual who cares about his own existence. Other writings stress that there is a meaningful world prior to individual consciousness in which the individual lives and in relation to which his consciousness emerges. In the later writings of Heidegger, for example, the world of appearances is described not as the creation of will but rather as a revelation of "gift" of Being to which men surrender themselves. Merleau-Ponty holds that the life-world in which we find ourselves is already there as a structured and meaningful world prior to our perception of its meaning. This is not to deny that our preconstituted world results from some prior human activity, such as that of a preceding generation, or that its meaning evolves and changes, partly as the result of our own actions.

There are overtones of Sartre and the early Heidegger in Jung's writings. He insists quite strongly that experience is not meaningful per se but that we endow it with meaning by our projects. Inasmuch as "reality is constituted by the meaning of our experiences,"[46] we must conclude that the real world is a construct of human consciousness. Nevertheless, Jung agrees with Merleau-Ponty that meaning or reality is not simply relative to individual actors. Action takes place within a world of shared meanings. Men who share a life-world or culture hold a common view of reality. The essential point, however, is that Jung would not hold that meaningful experience or reality is simply given to a passive consciousness, as is perhaps suggested by his references to our common-sense understanding of social and political things as "immediate" and "preconceptual." The social reality which the theorist attempts to describe is actively constructed in everyday life and thought.

Jung writes that the life-world is "dynamic and changing, which means that it is historical. As there are different cultures in history, moreover, there are different versions of the life-world."[47] It follows that views of reality attendant upon these different versions of the life-world are relative to time and place. Given these principles, it would seem that Jung can avoid an historical relativism only by granting that philosophy or science can transcend the horizons of particular versions of the life-world and discover permanent truths in nature or history. In keeping with the characteristic emphasis of existential phenomenology, he appears to deny the possibility of such transcendence. He criticizes those approaches to philosophy such as Plato's which look beyond the world of appearances in search of reality. The search for a permanent "nature" would be fruitless in any event, for nature has a history, i.e., natural objects are the products of involved consciousness and as such are inherently changeable. The task of theoretical reflection is to describe the meaning of experience as it is found in the life-world. Jung does suggest that the study of the life-world in its different versions would reveal invariant structures or essential types that lend themselves to theoretical formulation. One is reminded of Dilthey's effort late in life to locate the types of human behavior and stylistic forms through a comparative study of *Weltanschauungen*. Yet Dilthey's own teaching about the historicity of thought makes questionable the presupposition that would seem necessary to the search for a reliable typology, namely, the presupposition that a standpoint outside the flux of history can be gained for contemplating the variety of historical types. Jung's search for the essential structures and types of the life-world is likewise called into question by his insistence that science itself is a human project or *praxis* undertaken by human beings immersed in a particular life-world. The theorist is left with no solid base on which to stand.

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[43] Jung, "Political Relevance," p. 562.

[44] Jung, "Political Relevance," pp. 547–553.

[45] See Jung, "Leo Strauss's Conception of Political Philosophy," pp. 509–514.

[46] Jung, "The Political Relevance of Existential Phenomenology," p. 541.

Jung indicates that the invariant principles to be discovered by a phenomenological study of the life-world will concern the structure of human action and consciousness. At the same time, he denies that man has a constant and universal nature or, indeed, any permanently fixed properties at all.

> As an active doer, man is always in the making or on the way. He is what he makes of himself, and he molds his future by his action. Thus man has neither fixed properties nor a predetermined future. The idea that man is always an open possibility constitutes his radical historicity.[48]

Yet if there is no human nature, what basis is there for general assertions about the structure of action and consciousness or even about man's "being in the world"? There can be no assurance that patterns of life occurring in the past or the present will hold in the future. Even if there is no fixed nature of man to be discovered, general knowledge might yet be possible if man's historical development has moved according to inflexible laws that become visible to theorists in the culminating stage of history. Jung rejects even this Hegelian solution to the problem of epistemological relativism: "The events of history are the making of human consciousness and intervention rather than blind, impersonal forces beyond human control. Nothing is 'historically inevitable.' "[49] There is much to be learned from Jung's reexamination of the philosophical foundations of political inquiry, but he does not provide us with a genuine alternative to an historical relativism.

I have suggested that Nietzsche is the most important precursor of twentieth-century historicism. Until recently, American political theorists were quite hostile to Nietzsche, largely, it would seem, because of his reputation as an enemy of liberal democracy and a forerunner of fascism. In the past decade, however, Nietzsche's epistemological principles have been restated in a succession of books and articles by Henry Kariel,[50] who has emerged as the most important and effective spokesman for radical historicism in American political science. In Kariel's view, Nietzsche was first to give powerful expression to man's loss of faith in the possibility of final knowledge in the moral and political realm. His work, along with that of Freud and Mannheim a few decades later, discredited traditional political philosophy by "disclosing that it is impossible, in principle, to establish with certainty what is just and good for man in common."[51] Nietzsche's relativism is both epistemological and ontological. He teaches that everything, including man himself, undergoes development or change and that there is no realm of final truth or morality behind the flux of appearances.[52]

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[47] Jung, "Political Relevance," p. 543.

[48] Jung, "Political Relevance," p. 542.

[49] This quotation appears in the revised manuscript of "The Political Relevance of Existential Phenomenology," p. 13, but not in the version of the essay that appeared in *Review of Politics*.

[50] See especially Kariel, "Nietzsche's Preface to Constitutionalism," *Journal of Politics*, 25 (May, 1963), 211–225; *In Search of Authority* (New York: Free Press, 1964); *The Promise of Politics* (Englewood Cliffs: Prentice-Hall, 1966); "The Political Relevance of Behavioral and Existential Psychology," *American Political Science Review*, 61 (June, 1967), 334–342; *Open Systems* (Itasca, Ill.: Peacock, 1969): "Expanding the Political Present," *American Political Science Review*, 63 (September, 1969), 768–776; and "Creating Political Reality," *American Political Science Review*, 64 (December, 1970), 1088–1098. Kariel's position is developed comprehensively in his forthcoming book, *Saving Appearances*, which I have had the privilege to examine in manuscript form. Kariel here argues forcefully against the possibility of a final or objective insight into "nature" or "reality": "Our knowledge is inescapably fiction, illusion, myth, ideology, and rationalization." He calls attention once more to Nietzsche's influence on this view of knowledge, but the contribution of other thinkers, such as Hobbes, Kant, and Schopenhauer are also examined. His discussion of American pragmatism brings out its relativistic tendencies with particular clarity. A critique of Kariel's thought is offered in Marvin Zetterbaum, "Self and Political Order," *Interpretation*, 2 (Winter, 1970), 233–246.

Kariel adopts Nietzsche's thoroughgoing relativism as the point of departure for his own political theory, although his conclusions remind one more of Dewey, for example, than of Nietzsche. Since we cannot know what is good or just for men in common, the state must be indifferent to moral values and ultimate ends. Its proper concern is the means for insuring to each person the maximum freedom to formulate moral truth and create visions of the good which integrate narrowly partisan, private goods. While Nietzsche was quite willing for the creative man, the *Übermensch,* to impose his standards on lesser men, Kariel argues that a consistent relativism will recognize the equal claim of all human ends and pursuits. It will permit all men to expand their range of experiences, test their potentialities, and display themselves publicly in a variety of roles. Recognizing with Nietzsche that man creates himself, Kariel would democratize creativity: ". . . we should ultimately accept as democratic nothing less than a society all of whose members are active participants in an interminable process—*and who will not mind such activity*."[53]

Kariel's thought has a radical thrust which distinguishes it from the political conservatism of some historicists. If human thought is unavoidably shaped by prevailing forces, one might conclude that the appropriate political stance is to accept and affirm all that history has foreordained for one's time. Kariel, like Nietzsche himself, rejects this passive acceptance of the given. An acquiescent or ironical stance toward prevailing conditions is justifiable only if one knows with absolute certainty that those conditions are inflexible and unyielding. Short of such knowledge, we should "make history," i.e., test the limits of change by opposing established realities and certified truths. What we believe to be real is, in large measure, our own subjective creation. We design the world of phenomena and bestow meaning upon it.[54] When we acquiesce in established opinions and institutions, our creativity is blocked. None of us has the freedom any longer to be a person. We are prevented from enjoying varied and novel experiences, playing new roles, and discovering new potentialities. Positivistic political science, in particular, has identified the real with the observable, discredited speculation about alternative possibilities, and reconciled man to his present fate. In our capacities as citizens and as political scientists, we can know the limits that reality places on our freedom only by testing it, by forcing it to yield: "Taking risks, we must attempt to violate whatever is alleged to be reality, whatever equilibrium positivists certify to be real."[55] A political science worthy of the name will reconceptualize the world and act to implement its new visions. It will serve as an irritant, as an opponent of established systems and verities. Political science, as thus practiced, will serve both to increase our knowledge by disclosing the potentialities and limits of political life and to humanize the political sphere by enlarging the scope for creative action.[56]

We see that Kariel's political theory embodies a conscious, thoroughgoing relativism. Kariel understands clearly the implications of the epistemological principles that underlie postbehavioralism. He forces us to face squarely the question of whether or not a relativistic position is viable.[57]

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[51] Kariel, *In Search of Authority,* p. 5.

[52] See Kariel, "Nietzsche's Preface to Constitutionalism," which appears in revised form in *In Search of Authority,* pp. 7–24.

[53] Kariel, *Open Systems,* p. 73. Italics in the original.

[54] See especially *Open Systems,* p. 123, where Kariel states: "What we single out depends on us—or, more precisely, on the methodological conventions, conceptual frameworks, and boundaries we establish. And since frameworks, dimensions, and boundaries—reality-organizing principles—are man-made, the facts they expose are contingent on what we deem right and proper, on our norms. Our norms, in other words, structure reality. Expressing our dispositions, they dispose over facts—as well as of them. They give reality a significance it cannot otherwise have. Our inquiries, directed by our norms and our conventions, give meaning which reality does not previously possess."

[55] Kariel, "Creating Political Reality," p. 1091.

[56] Kariel, *Open Systems,* pp. 121–142; "Expanding the Political Present," pp. 772–776; "Creating Political Reality," pp. 1091–1098.

[57] I have put this question to Professor Kariel and received the following reply in personal correspondence, which I quote with permission: "Is a thoroughgoing relativism a viable position? Perhaps we'll have to redefine—to define—'position,' specify what something vacuous and literally pointless consists of, inquiring into the constitution of nothing. My only *ground* for not despairing of the groundlessness of the present is ignorance, my present conviction (daily reconfirmed) that I don't know what is ahead, that the

Conclusion: Beyond Positivism and Historicism in Political Inquiry

This essay has examined the controversy between behavioral and postbehavioral views of political inquiry in light of the larger dispute in modern philosophy over the character of human knowledge. We have seen that while behavioralism continues to be the dominant approach in American political science, its epistemological foundations have been dealt a severe, if as yet unperceived, blow by critics of positivism. It remains uncertain whether or not positivism, in some modified form, will continue to provide the guiding principles for political inquiry or, if not, where the new epistemological and methodological principles will come from. An increasing number of political scientists, in formulating their opposition to behavioralism, are drawing upon one variety or another of the antipositivist theory of knowledge that may be called historicism. Yet the capacity of this theory of knowledge to sustain a science of politics is, to say the least, problematical. Its principles lead to an epistemological relativism that renders questionable that very possibility of science or philosophy as understood in the Western tradition.

The conviction that neither positivism nor historicism can provide a satisfactory basis for political inquiry lies behind the renewed interest in traditional approaches to political philosophy.[58] Positivism and historicism have been allied in their opposition to traditional political philosophy, whose goal was knowledge of the good or just political order. Positivism bases its opposition on the claim that value-judgments cannot be verified empirically, while historicism insists that values are but individual or social creations, with no support in nature. It is possible, nonetheless, for proponents of positivism and historicism to support a reappraisal of earlier thought; for on some points of epistemology, each of these positions finds itself allied with traditional political philosophy in opposition to the other. Historicism agrees with tradition, in opposition to positivism, that evaluation is essential to understanding and, in particular, that the human things cannot be understood apart from judgments of good and bad. Conse-

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future is open and empty, that, while the past is a horror, there is still no evidence that the future must be the same. (If one is not caught in the conventional conception of time, one can of course think of the past as equally open, as not fully known, as not exhaustively horrible—and hence as allowing for hope.) So I am rather pleased by our lack of knowledge: my failure to know justifies these very words. To put it differently, I am in the position of expectancy (a feminine posture), anticipation, waiting. It arises out of my confidence in my ignorance—not my faith in the redeeming value of what may yet appear in the silences and spaces before us." He later adds: "I should still like to maintain not that relativism is 'viable' but that nothing else is."

[58] Leo Strauss and Eric Voegelin have been the most influential figures in the effort to restore political philosophy. For Strauss, see especially *Natural Right and History* (Chicago: University of Chicago Press, 1953), *What is Political Philosophy?* (Glencoe, Ill.: Free Press, 1959), *The City and Man* (Chicago: Rand McNally, 1964), "Relativism" in *Relativism and the Study of Man,* ed. Helmut Schoeck and James W. Wiggins (Princeton: D. Van Nostrand, 1961), pp. 135–157, and "Philosophy as Rigorous Science and Political Philosophy," *Interpretation,* 2 (Summer, 1971), 1–9. For Voegelin, see especially *The New Science of Politics* (Chicago: University of Chicago Press, 1952); *Order and History,* 3 vols. (Baton Rouge: Louisiana State University Press, 1956–1957); and *Anamnesis* (Munich: Piper, 1966). A fourth and final volume of *Order and History* is in preparation. For useful accounts of Voegelin's thought, see Gregor Sebba, "Order and Disorders of the Soul: Eric Voegelin's Philosophy of History," *Southern Review,* 3, New Series (Spring, 1967), 282–310; William C. Havard, "The Changing Pattern of Voegelin's Conception of History and Consciousness," *Southern Review,* 7, New Series (January, 1971), 49–67; Dante Germino, "Eric Voegelin's *Anamnesis,*" *Southern Review,* 7 New Series (January, 1971), 68–88; and Ellis Sandoz, "The Foundations of Voegelin's Political Theory," *Political Science Reviewer,* 1 (Fall, 1971), 30–73.

Both Strauss and Voegelin have defended political philosophy against the positivist critique. Strauss has gone on to identify historicism, rather than positivism, as the dominant force in contemporary thought and the leading contemporary opponent of political philosophy. His own work in classical political philosophy may properly be seen as a rebuttal of both the theoretical historicism of Hegel, which claims that political philosophy as quest for knowledge has been superseded by complete knowledge of the historical whole, and the relativistic historicism of Heidegger, which holds that views of the good must always be relative to groundless choice or to a dispensation of fate. Voegelin's stand with respect to historicism has not been as clear-cut as Strauss's although, he, too, is sharply critical of the thought of Heidegger as well as that of Hegel. In formulating his own position, Voegelin has drawn in some measure from the historicist tradition. He has held, for example, that political science in its full grandeur is philosophy of history and that the conditions of a civilization set limits to the capacity of its members to discern the truth about reality. Stanley Rosen has drawn attention to these aspects of Voegelin's thought in his review of *Order and History* in *Review of Metaphysics,* 12 (December, 1958), 257–276. I would contend, however, that the thrust of Voegelin's thought is away from historicism, at least in the sense of epistemological relativism. Of decisive importance is his view that man may, by the analysis of the experience of existence, grasp the truth about the order of being and, in light of this knowledge, rank the symbolic representations of reality that various civilizations have produced. There are, of course, other important differences between Strauss and Voegelin, such as their disagreement as to whether Political *philosophy* can or should be guided by revelation. Political theory or philosophy as Voegelin conceives it would probably be looked upon by Strauss as a type of political theology.

quently, the scientific method, as reconstructed by positivism, cannot disclose what is human about man and society. Positivism agrees with tradition, in opposition to relativistic epistemologies, that reliable knowledge of nature is possible in at least some areas. The traditional view had been that the mind can grasp nature as it is and, on the basis of such knowledge, apprehend what ought to be. Positivism came to deny that there can be genuine knowledge of what is good and just, or of the standards ("ideals," "values") that ought to guide political choice, but it continued to insist on the possibility of knowing the factual world as it is. By questioning the possibility of objective knowledge of both facts and values, historicism takes a radical step beyond positivism as well as beyond traditional theories of knowledge. It undercuts both positivistic approaches to political science and traditional political philosophy. Whatever their shortcomings might have been, traditional approaches to political philosophy, especially the ancient or classical approaches, encouraged a discriminating study of man and politics while preserving the possibility of reliable knowledge. The study of political philosophy in its traditional forms may thus contribute to the formulation of a mode of political inquiry that will do justice to the proper aspirations of positivism and historicism while avoiding their characteristic pitfalls.

In discussing the 'postbehavioral revolution" in political science, David Easton takes note of each of the positions that I have described. I would argue, however, that there are two fundamental errors in Easton's analysis of these positions and their relationships. He is mistaken, first of all, in thinking that the aims of the postbehavioral revolution can be reconciled with the effort to "convert the study of politics into a more rigorously scientific discipline modelled on the methodology of the natural sciences."[59] He argues that political science can accommodate the demands of postbehavioralism without surrendering the long-run objectives of behavioral science if it devotes more resources to applied research, acknowledges the influence of values on research, and engages in creative speculation about alternative kinds of political relationships that might be possible in the future. Easton fails to see that the postbehavioral revolution is heavily influenced by historicist epistemology and is, to that extent, opposed to "science" as behavioralism has defined it.

Easton's second error is to underestimate the importance and vitality of current efforts to restore political philosophy as the quest for knowledge of political things both as they are and as they ought to be. In fact, he regards them as unworthy of serious attention from political scientists because they are not "future oriented." According to Easton, they seek "to return to some golden age of political research" and, for that reason, require an adherent "to deny the possibility of discovering testable generalizations about human behavior." Those who adopt this viewpoint have, by their reactionary stand, "forfeited their opportunity and put in question their fitness to undertake th(e) creative task of theory" that the future requires.[60] I suggest that these efforts to restore political philosophy are best understood as a response to the social and intellectual crisis that Easton himself has spoken of in his early writings and again in his presidential address. Proponents of this restoration share Easton's early conviction that the crisis of modern thought and life is largely the result of a pervasive disillusionment with reason and that historicism is a primary manifestation of contemporary irrationalism. They are no less concerned than he is with contemporary society's predicament and with its future, but they differ on this point: they are able to consider traditional approaches and principles as possible sources of future guidance because they have freed themselves from Easton's implicit assumption that what arose in the past has been rendered obsolete by historical progress.[61] Efforts to restore political philosophy speak to the most urgent tasks in full awareness of the epistemological issues that these tasks must face. They cannot justly be accused of indifference to present needs and future tasks, but they may well be untimely, in Nietzsche's sense of the term. Their success may have to await the recognition by political scientists of the nature and implications of historicism, just as it awaited earlier a recognition of the limitations of positivism.

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[59] Easton, "The New Revolution in Political Science," p. 1051.

[60] Easton, "The New Revolution," pp. 1051, 1058.

[61] I have examined the development of Easton's thought, including his somewhat ambiguous position with respect to historicism, in "David Easton's Political Theory," *Political Science Reviewer*, 1 (Fall, 1971), 184–235. It is noteworthy that Ludwig von Bertalanffy, the leading figure in the establishment of general system theory, has developed a "perspectivistic" theory of knowledge, which adopts some of the central premises of historicism but seeks to preserve, in a limited sense, the possibility of absolute knowledge of reality. See Bertalanffy's "An Essay on the Relativity of Categories," *General Systems*, 7 (1962), 71–83.

Comment: Getting the War News Straight: The Actual Situation in the Philosophy of Science

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According to Miller, the forces of positivism lost the struggle for epistemology in this century to the forces of historicism some time ago; and according to him, the latest news from the philosophical front is that the flag of historicism has been raised within the positivists' last stronghold, the philosophy of science itself. The behavioralist program in political science thus rests on a doctrine about science that has now been entirely defeated; if they wish to be up-to-date philosophically, political scientists must come to terms with historicism.

It is hard to tell what wars Miller thinks he is talking about. Where did these great struggles take place? In philosophy the world over? In German philosophy? In English-speaking philosophy? For better or worse, the plain fact is that, except for the logical positivists themselves and a few philosophers who in part and for a time sympathized with them, there has been almost no serious exchange of views either in epistemology or in the philosophy of science between German-speaking philosophers and English-speaking philosophers during the past half century.[1]

The logical positivists, furthermore, and such figures as Popper and Wittgenstein, have led mainly English-speaking careers. They and their students and protégés dominate the scene in the philosophy of science in the English-speaking countries (to which we must add the Scandinavian countries), where it is more vigorously pursued by more philosophers specially trained to do so than in all the rest of the world put together. So historicism has not even occupied the field by default.

The Development of Postpositivist Epistemology under the Positivists' Auspices

It is true that the logical positivists long ago cast off some of the most striking theses with which they originally set forth; their present program for the logical analysis of scientific discourse is more complex and more sensitive than their original one. But if (as in part was the case) their original program was set up in conscious opposition to historicist writers, the positivists have had no occasion to refer to historicists since; and their students and protégés have never even been exposed to historicist writings. The classic criticisms of the original positivist theses have been produced by the positivists themselves; and the modifications of their original program have been brought about by self-criticism internal to the continuing positivist movement. In the early days of the Vienna Circle, the philosophers whom Miller cites as the founders of historicism served analytic philosophy as paradigmatic sources of what philosophy ought to avoid.[2] Eventually, the work of these philosophers literally dropped out of sight. So far as English-speaking philosophy is concerned, they remain to this day as nearly totally ignored as any philosophical movement could be. Almost no graduate student doing epistemology or the philosophy of science at an English-speaking university is ever exposed to them.

In the more recent history of the philosophy of science in the English-speaking countries, certain heretofore accepted claims and programs of logical empiricism have come under renewed scruitiny by positivists *themselves*, their own protégés and students. That is, recent philosophy of science is a product of positivists and philosophers sympathetic to positivism, and not a product of historicists or their successors on the Continent. For example, consider the development of the discussion of Carnap's concept of a logical confirmation function, i.e., a function which states for any given hypothesis and body of evidence the degree of confirmation of that hypothesis on the basis of that body of evidence; and makes that degree of confirmation of purely logical consequence of statements describing the evidence and the hypothesis in question. The development of this notion took up a very large part of the positivists' time (though Miller gives it no attention at all).[3] Today there is a large body of literature the burden of which is that the program of developing such a notion cannot succeed, because of the variety of terms which figure in scientific and mathematical propositions, their increasing numbers, the complexity of patterns of inference found in the natural sciences (e.g., the use of analogical arguments) and the limitations of extensional logic. This literature is exclusively the result of work by men like Popper, Nagel, Hempel, Salmon, Quine, and Putnam,[4] none of whom are historicists.

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[1] One might distinguish between weak exchanges, in which a philosopher or philosophers in the one camp read and commented upon writings that appeared in the other camp, without being noticed there or commented upon in turn; and strong exchanges, in which the other camp replied. There have been very few exchanges even of the weak sort, though Jürgen Habermas's rather comprehensive readings in English-speaking philosophy, as reflected for example in his *Für Logik der Sozialwissenschaften* (Frankfurt, 1970; the principal essay was published first in 1967 as a *beiheft* of the *Philosophische Rundschau*), deserve to be counted. In France, some philosophers of the structuralist school have been turning to writings in English-speaking philosophy, but not in a hostile mood: they cite Carnap, for example, with some favor. (Cf. the reference to Carnap in Alain Badiou's review article on Althusser, in *Critique*, Paris, 23, No. 240 [Mai, 1967], 438–467, at p. 450.) Antipositivism is not the latest news from the continent.

[2] Indeed, the products of some of these thinkers were cited by positivists as examples of literal nonsense. See, for instance, Rudolf Carnap, "The Elimination of Metaphysics through Logical Analysis of Language," in *Logical Positivism*, ed. Alfred J. Ayer, (Glencoe, Ill.: Free Press, 1959) (Carnap's essay was originally published in 1932) and Marhenke, "The Criterion of Significance" in *Proceedings and Addresses of the American Philosophical Association*, 1950.

Their work, and much of the revisionary work in the philosophy of science, is due to a more careful and detailed study of the actual practice of the natural sciences by analytic philosophers. It is not the result of a sudden awareness of "the historicity of the human mind" nor the result of a "reinterpretation of natural science in accordance with historicist principles," nor even the result of a "broader dispute at the level of epistemology about the character of human knowledge." In fact, the epistemological positions of postpositivist philosophers of science seem, more often than not, to be final reflections rather than starting points of their revisions of positivist doctrine. For example, Goodman's "new riddle of induction" is in considerable part the product of his work on Hempel's papers "A Purely Syntactical Definition of Confirmation"[5] and "Studies in the Logic of Confirmation."[6] Feyerabend's epistemological claims are historically the product of his study of the philosophical problems of the theories of relativity and quantum mechanics and of the realist/instrumentalist debate in the philosophy of science.[7] Hanson's claims about perception are chiefly the outcome of his reflections on the Copenhagen interpretation of quantum mechanics.[8] Though in each case the epistemological claim is the logical foundation of the claim about science, it is not the historical origin of the philosopher's claim.

The work of the eminent sometime logical positivist C. G. Hempel provides a good example of the epistemological vicissitudes of the original positivist program. Thirty years ago Hempel believed that it was in principle possible to distinguish cognitively meaningful from meaningless sentences by reference to a principle of verification.[9] Further, he had complete confidence in a distinction between analytic and synthetic propositions.[10] He was equally sure that a program for distinguishing theoretical from nontheoretical terms on the basis of observability would eventually succeed.[11] In the course of the succeeding years Hempel surrendered his belief in each of these central positivist doctrines. In the early 'fifties he began to display skepticism about the first of them in "Problems and Changes in the Empiricist Criterion of Meaning"[12] and in "The Concept of Cognitive Significance: a Reconsideration."[13] These papers proved to be the *locus classicus* for refutations of the verification principle. In Hempel's subsequent writings it is claimed that whether a term or sentence has cognitive significance is largely a function of the explanatory and predictive power, the formal simplicity, the clarity and precision, and the degree of experimental confirmation of the entire theory in which the term or sentence figures.

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[3] See, for instance, Rudolf Carnap, *The Continuum of Inductive Methods* (Chicago: University of Chicago Press, 1952) and *The Logical Foundations of Probability* (Chicago: University of Chicago Press, 1962.) These works summarize and extend work given currency by Carnap in articles over several previous decades.

[4] See, for instance, in P. A. Schilpp, ed., *The Philosophy of Rudolf Carnap* (LaSalle, Ill.: Open Court, 1963), articles by Ernest Nagel, Hilary Putnam, John Kemeny, and Arthur Burks; Sir Karl Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959); Wesley Salmon, *The Foundations of Scientific Inference* (Pittsburgh: University of Pittsburgh Press, 1968), for a simple account and criticism. Peter Achinstein's "Confirmation Theory, Order and Periodicity," and "Variety and Analogy in Confirmation Theory" in *Philosophy of Science,* 30 (1963), 17–35 and 207–221, respectively, present examples of very technical objections.

[5] Carl G. Hempel, *Journal of Symbolic Logic,* 8 (1948), 122–43.

[6] *Mind,* 54 (January 1945), 1–26, 97–121.

[7] See, for instance, Paul K. Feyerabend's "Problems of Microphysics" in *Frontiers of Science and Philosophy,* ed. Robert Colodny (Pittsburgh: Pittsburgh University Press, 1962); "Explanation, Reduction, and Empiricism" in *Minnesota Studies* (Minneapolis: University of Minnesota Press, 1962), 3, 28–97; "Realism and Instrumentalism" in *The Critical Approach to Science and Philosophy,* ed. Mario Bunge (Glencoe: Free Press, 1964); "How to be a Good Empiricist" in *Delaware Seminar in Philosophy of Science,* 2 (1962) and "Problems of Empiricism," Part I in *Beyond The Edge of Certainty,* ed. Robert Colodny (New York: Prentice-Hall, 1965), and Part II in *The Nature and Function of Scientific Theories,* ed. Robert Colodny (Pittsburgh: University of Pittsburgh Press, 1970).

[8] See, N. R. Hanson, *Patterns of Discovery* (Cambridge: At the University Press, 1965), and *Concept of the Positron* (Cambridge: At the University Press, 1963).

[9] See Hempel and Oppenheim, "Studies in the Logic of Explanation," *Philosophy of Science.* 15 (1948), 135–175. This conviction seems to underlie adequacy condition R3.

[10] See, for example, Hempel, "Geometry and Empirical Science," and "On the Nature of Mathematical Truth," *American Mathematical Monthly,* 52 (1945), respectively, 7–17, and 513–556.

[11] See Hempel's *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago Press, 1952).

Later in the 'fifties Hempel acknowledged the force of Quine's arguments[14] against the analytic/synthetic distinction. He moved in this direction in "The Theoretician's Dilemma."[15] The problems raised by Quine pose the gravest sort of challenge for the entire program of logical positivism, for the positivists held that philosophy consisted exclusively of analytic propositions. If no analytic/synthetic distinction could be drawn, the very existence of the discipline was thrown into doubt. Needless to say the origin of these problems, again, is not in the work of the historicists, though their solution is a continuing task for analytic philosophers.

More recently Hempel has reformulated his account of scientific theories in order to avoid the objection that no distinction between theoretical and nontheoretical terms can be drawn on the basis of observation. In "On the Standard Conception of Scientific Theories"[16] he eschews the view that nontheoretical terms are those which are defined by way of observable terms in favor of the view that nontheoretical terms are those which are comprehended by scientists previous to the introduction of the theory with respect to which they are denominated "nontheoretical."

Hempel's views, their changes, and the arguments for them are representative of recent developments in the epistemological concerns generated by the positivist program, as any philosopher in the discipline can attest. They are hardly in any obvious way the result of Hempel's discovering either that there is "no direct awareness of pure sense-data,"[17] or of his discovering "the historicity of the human mind,"[18] or "the relativity of truth."[19] Nor are these changes the results of exposure to Hegel, Nietzsche, Jaspers, Heidegger, Weber, Husserl, Dilthey, Ortega, Mannheim or any of their successors. The changes are the products of discussion and research by positivists and their own students.

The problem of distinguishing theoretical from nontheoretical terms is an especially important one in this connection. For today, apart from Quine's problems, the main challenge to positivism centers on this question, although Miller fails to recognize this, occupied as he is with grander disputes. The only real arguments which, in the eyes of philosophers of science, have even been brought forward in favor of Kuhn's views, *as Miller envisages them,* on the relations between old and new theories and paradigms, are those which allege that no distinction can be made between the theoretical and the nontheoretical terms of a theory, because the theory itself determines the meanings of all its terms. Therefore there are no intertheoretical inferences which do not rest on equivocations, and no rational evaluation of any given theory, on the basis of observation, or of some other theory, is possible. For example, Newtonian mechanics cannot be disconfirmed by reference to observable consequences or theoretical results of the special theory of relativity, since the meanings of crucial terms such as "mass" differ between the respective theories. Many versions of this sort of argument (due principally to P. K. Feyerabend)[20] have been shown (to the satisfaction of many philosophers) to be defective either by proving too little or too much. Regardless of whether his claim can ever be adequately grounded, its historical origin (again) is to be found neither among the continental philosophers Miller names, nor among historicists. It is the outcome of the thinking of positivists, their sympathizers, students, and (in the other analytic tradition) the followers of the later Wittgenstein.

It would be equally mistaken to suppose that

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[12] Hempel, *Revue Internationale de Philosophie,* 11 (January, 1950), 41–63.

[13] Hempel, *Proceedings of The American Academy of Arts and Science,* 80 (July, 1951), pp. 61–77.

[14] See "Two Dogmas of Empiricism" in Willard Van Orman Quine, *From a Logical Point of View* (Cambridge: Harvard University Press, 1953), pp. 20–46.

[15] Hempel, *Minnesota Studies in the Philosophy of Science,* 2 (1958), pp. 37–98, iv., sec. 7.

[16] Hempel, *Minnesota Studies in the Philosophy of Science,* 4 (1969), pp. 142–163.

[17] Miller, "Positivism, Historicism and Political Inquiry," *American Political Science Review,* this volume, this number, p. 800. Such a claim may play some part in arguments which lead to conclusions like the ones Hempel now espouses. But Miller fails to catalogue the precise roles it might play. And, in Hempel's case such a claim was not the product of any "historicist" thinking. Furthermore there is no essential connection between denials of the existence of sense-data and any of the views mentioned above; and positivists generally considered problems of the existence of sense-data immaterial to their program, practically from the outset of the movement.

[18] Miller, p. 800.

[19] Miller, p. 800.

[20] See references above, note 7.

Wittgenstein was himself a historicist or even influenced by the philosophers whom Miller so denominates. Certainly he never cited any historicist authors as sources for his work on language games. Although his discussions of them and of "forms of life" may invite attempts at an historicist interpretation, these are far from having become standard. Moreover, such an interpretation would run contrary to Wittgenstein's insistence upon the copresence in our culture of many language games in which we all participate without repudiating any as we change from one to another. In other words, language games are not so many Weltanschauungen; nor in giving way one to another do they necessarily or normally relegate one another to obsolescence.[21]

The points just made could equally well count in epistemology or in the philosophy of science (that specialized branch of epistemology). Let them count in epistemology. What could be more epistemological, after all, than the notion of a confirmation function, or the problem of relating theoretical terms and statements to pretheoretical ones? It is thus not true that historicism has gained ascendency over positivism in epistemology, at any rate in the English-speaking world; nor have the epistemological transformations of positivism been motivated by criticisms from historicists. The earlier phase of the struggle reported by Miller either never took place or had antagonists different from those he describes, and a different outcome.

The Positivists' Stronghold in the Philosophy of Science Retained

What about Miller's latest news of the fall or impending fall of positivism's last stronghold in the philosophy of science? Again his information is garbled. There has indeed been an uprising within the stronghold; but, contrary to Miller's report, the theses put forward by the rebels are not identical with the theses that he ascribes to historicists. Nor have their theses been animated by the influence of writers holding historicist theses. The theses are not, furthermore, antipositivist in any comprehensive way: they leave standing, for example, the outlines of the hypothetico-deductive method: and of the form of nomological explanations. Finally, though some concessions will no doubt be made to the rebels, they are very far from winning their main points.

Miller enumerates three claims of Shapere's[22] about postpositivist philosophy of science which to Miller are characteristic of it; and which he claims are historicist and antipositivist. First a "presupposition theory of meaning": the meanings of all scientific terms are determined by the underlying paradigm or theory. But surely *acceptance* of this claim could not be characteristic of contemporary philosophy of science since it is in fact one of the most hotly disputed theses in the current literature; and in the bald way Miller expresses it, the claim has been refuted to the satisfaction of a large number of philosophers. Indeed, the work of Shapere's to which Miller refers presents a well-known attack upon this view. Most important, it has yet to be demonstrated that if the meaning of terms were dependent upon the theories in which they figure, this dependence would entail or even give substantial grounds for believing that theories are incommensurable; paradigms, unbridgeable; scientific claims, not subject to rational disputation; truth, relative: or for believing any other historicist claim.

The second of these claims is the claim that "what can count as an explanation" may change as different problems come to "define the domain of scientific inquiry." Shapere does not explicitly dilate on this claim, but Miller suggests that it is the same as claiming the logical character of explanation varies over contexts. This is not, however, the only interpretation possible or plausible. Taken as a consequence of Kuhn's views, it appears more reasonable to interpret the claim as merely making the point that the sorts of items that can adequately explain phenomena differ over theories. This is a view that positivists could accept. Moreover, regardless of how the claim is interpreted, it is not the same as the view that "the human mind is essentially historicist," and not the same as the claim that truth is relative. Indeed, any argument which proposes to move from the context-dependent character of the concept of "explanation," as scientists use it, to the claim that the concept of truth, as scientists understand it, is context-dependent seems doomed from the outset. The adaptive nature of variations in the character of scientifically acceptable explanations, in their contents, and in the criteria of adequacy which scientists apply to them seems to indicate that so far as the actual pursuit of science is concerned, the concept of truth is very far from relative. Scientists seem to take succeeding scientific explanations of the same phenomenon to have the character of successively more accurate approximations to some nonrelatively true account of the explanandum.

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[21] See Ludwig Wittgenstein, *Philosophical Investigations,* trans. G. E. M. Anscombe (Oxford: Basil Blackwell, 1953), for example. Sect. 18, p. 8, and Sect. 23, pp. 11–12.

[22] From Dudley Shapere, "Meaning and Scientific Change," *Mind and Cosmos,* ed. Robert Colodny (Pittsburgh: University of Pittsburgh Press, 1966), pp. 41–85.

The third claim which Miller draws from Shapere as characteristic of antipositivist philosophy of science is expressed in the following manner: "The antipositivists adopt a presupposition theory of the relevance of facts to theory, of the degree of relevance (i.e., of the relative importance of different facts) and, generally, of the relative acceptability of different scientific conclusions. . ." This citation by itself is too general and vague to give any clear idea of what *Miller* has in mind; *Shapere* makes it clear that the claim is directed primarily at Carnap's claims for a purely formal confirmation function. But the serious arguments against Carnap's views are all the products of positivists' rethinking and their students' insights. Moreover, the rejection of this view is logically unconnected with the rejection of other positivist claims, such as the hypothetico-deductive view of theories and laws, and the deductive-nomological account of explanation; nor does this third postpositivist claim provide any ground upon which to deny the importance which positivists attached to the confirmation of scientific propositions.

In the case of some of these claims it is not clear that they are incompatible with positivism, and in the case of others, there is no general agreement on their truth by philosophers of science. But none of them is taken by their defenders or their objectors to be "reinterpretations of modern science . . . in the light of . . . historicism." More important, it is false to say that these assumptions entail the historicist view, without the provision of further arguments and a large number of intervening steps; arguments and steps which, to the satisfaction of *most* philosophers of science, have not yet been adduced.[23]

The Repudiation of Relativism, and The Reformation of a Relativist

An uncritical blanket acceptance of Kuhn's views, and their theoretical underpinnings, especially as adduced by P. K. Feyerabend, has led some social scientists to suppose that no fundamental, nonarbitrary rational standards can be adduced for the final assessment of any theoretical work. It is indicative of the misguided nature of Miller's account of the present situation in the philosophy of science that while these social scientists seem to be correct in drawing radically relativist conclusions from incautious and summary assertions of Kuhn's work, supplemented by Feyerabend's claims about the meanings of scientific terms, most philosophers of science take these same conclusions to be a *reductio ad absurdum* of the theory in its simplest form.

This is not the place to go into general arguments against unguarded relativism, of the sort licensed by the third of the theses that Miller ascribes to historicists: the relativity of truth. But it can hardly be said that these arguments have been discredited. How, for example, can the position that truth is completely relative to culture be reconciled with the existence of innumerable possible counterexamples of statements that seem equally verifiable in all cultures? ("You're standing on my foot," "It's raining," "The fire has gone out.") Again, are the conceptual schemes of different cultures supposed to be intertranslatable? If they are, then it appears that the different cultures can share truths and observations after all; on the other hand, if the schemes are not intertranslatable, it would appear impossible to demonstrate that the truths found with one differ in meaning from the truths found with the other.[24] Differences between cultures are not in fact incompatible with acceptance by one culture of truths found by another: medieval culture was very different from the culture of Greco-Roman antiquity and ours very different from both; but medieval culture shared a body of knowledge with the Greeks and Romans and we share a body of knowledge with both, a good deal of which we have inherited from them.

Hegel and Marx, of course, were writers who could hardly have been more conscious that knowledge accumulates as one culture succeeds another, at least when the succession takes place in reasonably favorable circumstances. They therefore had very good reasons in mind for not adopting the third, most controversial of the historicist theses. But the thesis and its implications would be as vigorously repudiated by the pragmatist writers, like Dewey, whom Miller would class with the historicists. In particular, pragmatists would have little patience with the suggestion that the first principles of knowledge are arbitrary. Pragmatists like Peirce and Dewey have characteristically believed with Hegel and Marx and most of the rest of us that there has been an historical evolution of concepts, principles, and theories toward better adaptation to the universe in which we happen to find ourselves. This belief, applied by Quine to logic and the natural sciences, is one in which he and the pragmatists are joined by Austin, who relied on it in his defense of ordinary language and its concepts.[25] How could any belief be farther removed from conceding to unrestrained relativism the "arbitrary" nature of our conceptual scheme? What is arbitrary about a scheme that has evolved by successively more precise adaptations to the universe?

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[23] See, for instance, Israel Scheffler, *Science and Subjectivity* (Indianapolis: Bobbs-Merrill, 1967).

[24] We owe this point to Kendall Walton, *Conceptual Schemes: A Study of Linguistic Relativity,* a Cornell doctoral thesis in 1967, available from University Microfilms. Walton's thesis and a slim volume on *Categorial Frameworks* by Stephan Körner (Oxford: Basil Blackwell, 1970) are the two most notable recent treatments of the basic, but curiously neglected question, "In just what ways can conceptual schemes differ?"

Philosophers have presented much more specific sorts of arguments against the views broached in the philosophy of science which seem to Miller to license the same relativism that historicism involves. Moreover, there has been a good deal of agreement among philosophers about the soundness of some of these arguments. In particular, the following consequences of the Kuhn-Feyerabend view are considered by many philosophres of science to be sufficiently absurd to refute the view in its present form:[26]

> (1) If the meaning of a term in a theory is dependent upon the principles of that theory, then no two theories can contradict each other. Thus, e.g., Van Der Waals' theory of gases is not, as previously supposed, incompatible with the classical kinetic theory of gases.
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> (2) It will be impossible for theorists to agree on a description of the data to be explained by their respective theories, for the terms in the descriptions will be dependent for their meanings on the differing theories. With neither agreement nor disagreement possible, it is hard to see how theories can even be alternatives to one another: that is, the Bohr theory of the atom is not at all about the same thing as modern quantum atomic theory.
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> (3) If the principles of a theory give the meaning of the terms which enter into it, then these statements can be understood as analytic truths, and theories could never be overthrown, no observed item could be described as failing to satisfy the theory, and the existence claims of the theory would be irrefutable as well. Thus, nothing could have refuted the phlogiston theory, or the supposition of spontaneous generation.
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> (4) If this doctrine were true it would be impossible for a person to learn a theory by having it explained through terms whose meanings he understands antecedently (i.e., by knowing the propositions of another theory). The only ways to achieve comprehension of the theory would be ways which somehow avoid the use of words. If this doctrine is correct, the notion that the pursuit of science is cumulative, that publication of new theories adds to our stock of scientific understanding, indeed the whole present view of the character of scientific education rests on a conceptual mistake.

These results are absurd. They are recognized as absurd because they are patently incompatible with examples from the history of the actual pursuit of science, examples which could be multiplied almost without limit.

The absurdity of these results do not necessarily invalidate weaker forms of a Kuhn-Feyerabend claim. But their absurdity certainly seems sufficient to refute that form of the thesis which supposedly licenses whatever view a scientist may espouse, provided he claims it represents a new paradigm incommensurable with its predecessors. Certainly Kuhn himself never envisaged his doctrine as granting methodological *carte blanche,* or entailing the sort of relativism which historicism seems to sanction. Miller reports Kuhn's tendency to believe that paradigms are not accepted or rejected "on rational or logical grounds."[27] But do not further remarks of Miller's lead one either to wonder whether this is not an overstatement of Kuhn's position, or to suspect that Kuhn's position is self-contradictory? For Miller also attributes to Kuhn the following view: "If there is to be a scientific revolution scientists must become aware of some anomalous fact that the old paradigm cannot explain. In this time of crisis for normal science, new theories are invented to account for the anomaly, one of which eventually gains wide acceptance as the core of a new paradigm." This passage makes it sound as though there are at least some rational grounds for rejecting the old paradigm, and some (other) rational grounds for accepting the new one. In the former case, the old theory's inability to account for some phenomenon; in the latter case, the new theory's ability to account for it.

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[25] Willard Van Orman Quine, "Two Dogmas of Empiricism," in his *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press, 1953; 2d ed., 1961), pp. 20–46, esp. p. 42ff. J. L. Austin, "A Plea for Excuses," in his *Philosophical Papers,* ed. J. O. Urmson and G. J. Warnock (Oxford: The Clarendon Press, 1961), pp. 123–152, esp. p. 133.

[26] Cf. Achinstein, *Concepts of Science* (Baltimore: Johns Hopkins Press, 1968); Shapere, "Meaning and Scientific Change," and others.

[27] Miller, p. 805.

Indeed, in Kuhn's more recent writing he has plainly rejected that interpretation of his work which legitimizes methodological anarchism. Referring[28] to the work of Sir Karl Popper, Kuhn writes:

> . . . Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts . . . [He] would not be the first if he mistook what I call normal science for an intrinsically uninteresting enterprise . . . [N]either science nor the development of knowledge is likely to be understood if research is viewed exclusively through the revolutions it occasionally produces. For example, though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing . . . Finally, and this is . . . my main point . . . normal science . . . rather than extraordinary [revolutionary, paradigm-shifting] science . . . distinguishes science from other enterprises.

In other words, the practice of science is not the constant provision of new paradigms whenever a scientist is dissatisfied with established ones, but rather a slow process of digesting, rationally evaluating and testing the ramifications of given theories, a process which is broken up by revolutions only every few centuries, and not in every issue of a learned journal. In Kuhn's view, then, science is characterized chiefly by research practice as positivists and postpositivists have described it, i.e., normal science. It is not distinguished from other endeavors by the occasional paradigm-shifts which so excite the historian of science. What is more, in Kuhn's latest view the alternative paradigms are not necessarily incommensurable, so that the possibility of translation between them exists:

> . . . [C]ommunication breakdown [between theories and paradigms] . . . needs much study. Unlike Paul Feyerabend . . . I do not believe that it is ever total or beyond recourse. Where he talks of incommensurability *tout court,* I have regularly spoken also of partial communication, and I believe it can be improved upon to whatever extent circumstances demand and patience permit. . .[29]

But if this is the case, it cannot be inferred from the existence or possibility of alternative paradigms that all observations and all claims to truth will be relative or historically bounded.

A Proposal for Producing More Instructive Criticisms of Behavioralism

Without giving personal offense to Miller or other political scientists with similar interests, may we conclude by making a plea for the extinction of the genre of which Miller's paper is a more acceptable example than most? It is not good political science, and it is not good philosophy: it is paraphilosophy, thinking that makes pronouncements about philosophy, but makes no contributions to it. Vast themes are broached, and dramatic things are said about them, but no inquiries are brought any nearer to a satisfactory end. Nothing is accomplished except perhaps the provision of a pretext for further work of the same kind, of which there is already altogether too much, considering that there are all sorts of actual political phenomena crying out for study or for restudy along lines that antibehavioralists might want to suggest.

For we by no means imply, when we refute rumors that the positivists have vanished from the philosophy of science and left the field to historicists, that everything which the positivists have inspired, in political science or elsewhere, has been well conceived, or that no useful guidance for political science or anything else can be drawn from historicist writers. If behavioralist studies ignore "the expressive meaning" and "the documentary meaning" of human activities, they need to be supplemented by studies that do not.

Supplemented, not supplanted. Is it reasonable to think that *nothing* can truly be said about human activities by behavioralist studies inspired by methods and concepts of explanation that have been successful in the natural sciences? Whether supplanting or supplementing is in question, however, the defects or omissions of behavioralist political science need to be brought home by matching particular behavioralist studies against studies, otherwise inspired, of the same phenomena; or at least by careful critiques of particular behavioralist studies. It is noteworthy that there are no such comparisons or critiques in Miller's paper. In how many of the articles or books that he cites by antibehavioralist political scientists are they found?

Their absence may be a considerable loss. It is interesting in this connection to recall that the best philosophical work done by natural scientists has been addressed to particular conceptual problems arising within their own scientific work. It has not consisted of general reflections evoked by the standard program of questions current among philosophers of science, though sooner or later that program has felt its impact.

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[28] Thomas Kuhn, "Logic of Discovery or Psychology of Research?" in *Criticism and the Growth of Knowledge,* ed. Imre Lakatos and Alan Musgrave (Cambridge: University Press, 1970), p. 6.

[29] Kuhn, "Reflections on My Critics," in Lakatos and Musgrave, p. 232.

For example, physicists like Bohr, Born, Heisenberg, Landé, Margenau and Schrödinger have made important contributions to philosophical discussions of causality, determinism, probability, and to the dispute between realism and instrumentalism in consequence of their researches in quantum mechanics.[30] The hypothetico-deductive model of scientific theories is heavily indebted to the philosophical work of N. R. Campbell, whose book, *Physics the Elements* (1920),[31] was the product of his own work in the physical sciences. And, of course, contemporary philosophy of geometry, space, and time, is largely the product of the philosophical writings of scientists like Poincaré and Einstein, writings which were antedated and motivated by scientific productions. It would be easy to extend this list of examples to several more topics in the philosophy of science.

One might reasonably expect that political scientists, too, are most likely to make genuine contributions to philosophy as incidents of their own renewed efforts to make sense of particular political phenomena. Simon's work on causality, on "satisficing" and on the distinction between "optimizing" and "adaptive" behavior can be cited as cases in point.[32] Might not antibehavioralists imitate him at least in this respect, and make sure that their criticisms are associated with particular studies revealing their different views of phenomena?[33] For example, take Simon's finding that the degree to which two kinds of specialists within a formal organization understood one another varied with the frequency with which they communicated; or Francis's finding that factional conflict within the majority party in a legislature is higher when it holds less than 60 per cent of the seats than when it has between 60 and 70 per cent or more than 90 per cent, a phenomenon that Francis attempts to explain by the use of coalition theory.[34] What would antibehavioralists find in these connections that would contradict these findings; overshadow them in importance; or at least differ from them? And how would the antibehavioralists' findings and explanations reflect conceptions of human activity which Simon's and Francis's views or methods prevent these authors from using?

When we plead for the extinction of paraphilosophy, we are not suggesting that contributions to philosophy from political scientists cease. We are suggesting that the contributions are most likely to be interesting philosophically when they are rooted in the study of actual political phenomena and in turn visibly feed the growth of such study. Nor are we suggesting that political scientists should cease to read philosophy and draw lessons from it. We are pleading for a looser attachment to philosophical doctrines.

Why should a political scientist think it necessary to choose sides for or against positivism? As an outsider, he is even more likely than philosophers themselves to get the proportions of the issues wrong. He cannot expect to follow a doctrine through all its transformations: or to identify precisely the key issues that lead from one transformation to another.

Can he even tell when it is a question of choosing sides? In the present context, to meet Miller on his own ground and simplify our exposition, we have freely used the terms "positivism" and "positivists"; but philosophers—at any rate, philosophers of science in the English-speaking world—do not think of themselves as positivists or even as postpositivists. They sometimes use the term "positivism" to designate doctrines left behind, several transformations ago, in the stream of mixed doctrines now current as evolutionary decendants of "positivism." But they think of themselves as holding one view of induction, shall we say, as opposed to the views held by some of their colleagues; or a particular position, one among several positions current, on the relation between explanation and prediction. What they are agreed on—the outlines of the hypothetico-deductive method; the form, waiving unresolved difficulties about the nature of law-like statements, of nomological explanations—does not define a side in current debate, not even a side against "historicists" or other antibehavioralists, who could hold all three of the doctrines by which Miller defines "historicism" and still accept Popper's account of the hypothetico-deductive method and Hempel's (also Popper's) account of nomological explanation.

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[30] See, for example, Niels Bohr, *Atomic Theory and the Description of Nature* (New York: Macmillan, 1934), and "Causality and Complementarity," *Philosophy of Science*, 4 (July, 1937), pp. 289–298; Max Born, *The Natural Philosophy of Cause and Chance* (Oxford: Clarendon Press, 1949); Werner Heisenberg, *The Principles of the Quantum Theory*, trans. Carl Eckhart and Frank C. Hoyt (Chicago: University Press, 1930), *The Physicist's Conception of Nature*, trans. Arnold J. Pomerans (New York: Harcourt, Brace, 1958), and *Philosophical Problems of Nuclear Science*, trans. F. C. Hayes (New York: Pantheon, 1952); Alfred Landé, "From Dualism to Unity in Quantum Mechanics," *British Journal for the Philosophy of Science*, 10 (May, 1959), 16–24; Henry Margenau, "Reality in Quantum Mechanics," *Philosophy of Science*, 16 (1949), 287–302; and "Conceptual Foundations of Quantum Theory," *Science* (January 26, 1951), 95–100, Erwin Schrödinger, "Die Gegenwärtige Situation in der Quantenmechanik," *Natürwissenschaften*, 1935.

[31] Now published as *Foundations of Science* (New York: Dover, 1957).

[32] See Herbert A. Simon, *Models of Man* (New York: Wiley, 1957); *Organizations* (with James G. March) (New York: Wiley, 1958); and (as a further example) Simon's article, "Some Strategic Considerations in the Construction of Social Science Models," in *Mathematical Thinking in the Social Sciences*, ed. Paul F. Lazarsfeld (Glencoe, Ill.: Free Press, 1954), pp. 388–415.

[33] It is fair to say that the contributors to the Storing collection (Herbert J. Storing, ed., *Essays on the Scientific Study of Politics* [New York: Holt, Rinehart & Winston, 1962]) did associate their criticism with particular studies, though with particular studies by Simon and other behavioralists rather than with contrasting particular studies of their own.

[34] See Simon's article in the Lazarsfeld collection, cited in footnote 32 above; and Wayne L. Francis, *Legislative Issues in the Fifty States: A Comparative Analysis* (Chicago: Rand McNally, 1967), p. 34ff.

Behavioralist political scientists, in their eagerness to profit from useful precedents in the natural sciences, may have attached themselves too rigidly to the early doctrines of the positivist movement. Miller may be right in suggesting that they did; but he has not given examples of particular studies gone astray, or proof that studies otherwise conceived, which would have returned more significant results for the same investment of effort, have been forestalled. Without such evidence, which Miller is not alone in omitting, it is difficult to tell what the consequences of this rigidity have been, or how harmful. It is not even clear that the behavioralists (by contrast with the antibehavioralists, some of whom seem to do nothing else) have spent too much time propagating their methodological views as against carrying particular studies forward.

In any case, it would be a misfortune for political science to exchange one excessively rigid attachment for another. Rigid historicism is as likely to be self-defeating as rigid positivism. The disappearance after refutation of early positivist doctrines does not entail the undesirability of applying some of the concepts and some of the methods of the natural sciences to political phenomena. On the contrary, disappearance of those doctrines opens the way to reconsidering this question. The behavioralists may have been quite right about the legitimacy of imitating the natural sciences (at least in part), though mistaken about what has to be done to imitate them correctly. But what in particular has been done wrong or wrongly left undone in consequence of this mistake? In all friendliness, may we not ask whether Miller and also Gunnell, Wolin, Connolly, Surkin, Kariel, and all, would not be better occupied in detecting and demonstrating the errors and omissions with which particular political phenomena have been misrepresented under behavioralist auspices, than in trying to assess the outcome of a struggle which some of them at least have not even been able to locate?

Comment: On Evolving Standard Views in Philosophy of Science

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Introduction

I have about two and one-half agreements with the account[1] Professor Miller puts before us. I agree with him that it is sensible for serious social scientists to be *au courant* in contemporary philosophy of science (and, indeed, other aspects of philosophy).[2] I agree with him that the past few decades have been a period of ferment and development in philosophy of science—and in epistemology generally—the results of which are worthy of study since, among other things, they may contribute to the refinement of our understanding of complicated issues in the logic of social inquiry. I have a severely qualified agreement (a "half agreement") with him about the *historical* "bent" (but not the *historicist* bent!) of *one* of the interesting lines of development in recent philosophy of science.

But I'm afraid our points of significant agreement go no further. I find the rest of the views he presents egregiously mistaken. And, since these mistakes may be misleading to those not familiar in detail with the character of the very complex developments of contemporary philosophy of science, it seems reasonable to do what one can, in even brief compass, to dispel the effects those views, unchallenged, might have.

Let me indicate the main import of his essay: Miller purports to bring word of the ascendancy, over positivism, of a view he calls *historicism* in epistemology and philosophy of science. This ascendancy, he also appears to be claiming, may be news to political scientists (those perennial culture laggers!) but it is presumably old-hat to philosophers; for, we are told, "by 1950 positivism was virtually dead as a philosophical movement." Moreover, the "demise" of positivism means that the once flourishing "behavioral approach" in political science is doomed. For, *its* "epistemological roots lie in positivism." His suggestion is strong that "behavioralism" doesn't quite seem to know enough to lie down. But the fruits of "ascendant historicism" are bitter too; and after a consideration of its manifestations in "postbehavioral" political science, Miller concludes that "the capacity of this theory of knowledge [historicism] to sustain a science of politics is, to say the least, problematical. Its principles lead to an epistemological relativism that renders questionable the very possibility of science or philosophy, as understood in the Western tradition." In a brief concluding turn, however, "the study of political philosophy in its traditional forms" is held before us by Miller as the means of salvation.[3]

[3] A main thrust of the essay is surely an attempt to show how what it construes as the triumph of historicism in philosophy of science has influenced "postbehavioralist historicist" movements in political science. However, by the time he has reached the antepenultimate paragraph, Miller has come to the conviction that neither the behavioralists nor the postbehavioralists represent viable approaches to the science of politics; for neither the positivism nor the historicism which respectively lie at the roots of these positions (so he claims) "can provide a satisfactory basis for political inquiry" (p. 816). He concludes with a critique of David Easton's recent discussion of behavioralist and postbehavioralist positions. I shall eschew any *detailed* comment on this final part of Miller's essay—his colleagues in the discipline itself may appropriately take account of his closing remarks. I will, however, simply mention what a surprising turn his final revulsion against historicism represents to anyone who, like me, had come to think of historicism as the "hero of the piece" on the basis of the earlier part of his paper. Yes, on the basis of the earlier portions of the paper, I had been reading him as a happy partisan of historicism. What we come to realize at the end, though, is that we are really being proffered a dialectical analysis. The positivistic thesis is, to be sure, superseded by the historicist antithesis. But the latter, too, offers political science no genuine basis and we must therefore pass to a (lower?) synthesis: Traditional Political Philosophy. Here is our real if somewhat neglected hero.

Of course, as might be noted, it's not always neat or easy to construe this dialectic as a "historical process." When positivism is identified with Hume and historicism with Kant (or perhaps Neo or Post-Kantians or Hegel or Neo- or Post-Hegelians) it's easy enough to see that positivism is the thesis and historicism the antithesis. But when historicism is identified with such 19th century proponents and positivism is identified with the Vienna Circle critics of these metaphysicians, one might think, conversely, that it was historicism that was the thesis and positivism the antithesis.

Moreover, when we pause to think that Traditional Political Science, harking back at least to the Middle Ages, must surely antedate both the "thesis" and the "antithesis," however construed, we might experience a slight twinge of puzzlement. But this puzzlement

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[1] Eugene F. Miller, "Positivism, Historicism, and Political Inquiry," *American Political Science Review*, 66 (September, 1972).

[2] Just as it is sensible for philosophers of science to be *au courant* in contemporary developments in the social sciences.

In fact the turn taken by Miller's essay makes it somewhat puzzling to deal with—at least with respect to its substantive philosophical views, in contrast to its historical views. Consider the two aspects separately. The essay's historical views concern the tenor of recent developments in epistemology generally and philosophy of science in particular. They offer the unpersuaded critic only the problem of documenting the actual course of recent developments in sufficient detail to cast doubt upon the soundness of those views. I believe, in fact, that Miller has made a drastically mistaken reading of the current and recent situation in philosophy of science. I present my reasons for this claim below.

On the other hand, the puzzle about substantive philosophical views he is seriously putting forward or espousing is troublesome for it is a puzzle about what his substantive philosophical views actually come to. For example, the theses he identifies as historicist are notoriously untenable. They are, to begin with, corrosively self-vitiating; (i.e., were they "sound" they would render their *own* soundness dubious). But they are defective as theses also inasmuch as they engorge a confused notion of the nature of objectivity and of the function of scientific accounts of the world. If one concluded that science fails of objectivity on the peculiar grounds which *that* historicism advances, one would, among other things, be equating objectivity with *conditionless* observation or conceptualization, one would be confusing scientific accounts of the world with reproductions of the world, one would be confusing the problem of formulating and accepting theories on always incomplete evidential bases with the problem of attaining infallibility in the acceptance of theories. All of these things are, in the literature, well known, thoroughly explained, crippling defects of historicist positions such as the one Miller outlines. In fact, that historicist position *shares* crucial aspects of this inchoate view of objectivity with the caricature of positivism which Miller delineates as its defeated opponent; (e.g., that version of positivism also sought unconditioned observation as an essential ingredient of "objectivity"). The difference here is simply that the caricature of positivism holds that science should and does attain objectivity, while historicism denies that science, or any other human cognitive endeavor, can do so.

But where is Miller in all of this? Does he subscribe to or credit cogency to this historicist view of objectivity or its concomitantly self-vitiating relativism? We are led (perhaps misled) into thinking so, right up to the final paragraphs of his essay when he proffers a third alternative to behavioralism and antibehavioralism in political science. But up to that point, the strong suggestion has been that historicism has conquered the positivist view in philosophy of science by dint of the superior cogency of its theses or arguments. By cleaving to a third view, Miller throws this interpretation into doubt. And, since he does cleave to that third view at the end, it would perhaps be unfair to impute to *him* the outworn relativist historicism which has been taken apart over and over again in the literature of analytic philosophy and philosophy of science. I assume, then, that there is no point in flogging the particular historicist views on objectivity that Miller presents. Accordingly, save for a claim about the "logic" of the connection between Miller's version of positivism and behavioralism in political science which I shall examine later, I shall keep discussions of Miller's substantive philosophical views brief, and, in the main, confine myself to a critique of Miller's "historical" theses—his reading of the current and recent situation in philosophy of science.[4]

My remarks will be addressed, then, to (1) the contention that recent developments in philosophy of science are properly characterizable as the triumph of historicism over positivism;

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doubtless stems from a too naive view of the dialectic. The dialectic isn't bound by mere chronology. We get to know "who's who" not by a mere round-by-round chronology, but by looking at the point-totals on the referee's score card. Never mind which rounds historicism "took"; all we need to know is that it won the decision—or at least that it would have won except that it had to be disqualified—as too punchy—too *non compos*—to carry the crown.

[4] It should be pointed out that Miller's essay and, perforce the present criticism of it, both fail to take adequate account of some genuinely exciting problems about objectivity which currently *do* concern philosophers of science. In the main, these problems pivot on the nature of language—problems of meaning and reference—of *translation* and of *coming to understand* some linguistic context. But the chief contributors to articulation and advance on these problems are *not* "Millerian historicists" but rather people who, like Quine and Chomsky, are as far removed from such nineteenth-century antiscientific miasmas as could easily be imagined. In short, not the least misleading aspect of Miller's account of recent philosophy is that it omits consideration of just what *have been* among the most vital of philosophical developments on meaning and objectivity. Another, and also vital, development vis-à-vis objectivity which Miller *does* mention, is one that has received greatest impetus in the work of T. S. Kuhn. But the usefulness of Miller's account is diminished here, too; for, as is indicated below (see, for example, footnote 10) Miller makes an erroneous assimilation of Kuhn's views to the historicism he has been outlining.

and (2) the contention that "behavioralism" in any science and, *a fortiori,* in political science, has its epistemological roots in positivism (even as that position is represented by Miller).

Miller's provocative claims about what has happened in philosophy do provide strong temptation to make extensive rejoinders concerning the actual history of the development of the "standard" view in philosophy of science—and to discuss some of the technical changes which have figured in that development. For the most part, since succumbing fully to this temptation would be inappropriate in the present context, I have tried to resist it; and where I was patently unable to do so, I have often, at least, compromised by pushing large portions of my commentary into footnotes. (I apologize for their consequent unwieldiness—but they may be helpful to some readers, and they can easily be skipped by anyone who finds them telling him more than he cares to know about all of this.)

Now, to begin quite generally, perhaps the most egregious of the essay's mistakes is to characterize the developments in philosophy of science over the past few decades in terms of conflict between positivism and historicism. I think I can convey quickly, and at the outset, how bizarre such an analysis is. It is somewhat as if one were to analyze current complex political developments in Italy in terms of conflict between Guelphs and Ghibellines.[5] It is not that it would be absolutely impossible for someone with immense ingenuity (who emphasized strongly, say, the political consequences of the present secularist-clerical quarrel about the new divorce laws) to couch an analysis in such terms. It is just that it would involve a breathtaking, procrustean insensitivity to do so; and it would be dreadfully misleading to people innocent of how the recent political situation in Italy has actually developed. In the following sections I shall try to show that Miller's construal of the historical and present situation is just of this order of implausibility.

Miller's Interpretation of Current Philosophy of Science

Miller's analysis of current philosophy of science contains errors both of omission and commission. The best way to support this blunt charge would be to present directly the historical and current evidence of what is, and has been, going on in the field. And the best way to do this would be to bring forward copious quotations and a very detailed discussion of how the ramified nature of many of the field's main problems has evolved into what a "snapshot" view or, perhaps, a cross-section of it, would make it look like today. But such a course might run to volumes and I shall have, for the most part, instead, to resort to giving directions for locating the copious evidence that may be found.[6]

Since my discussion must be relatively brief, though, it will be sensible to organize it around a small number of general topics which have been, and continue to be, of pivotal concern during the past few decades. Let us then consider developments during the period of about the last fifty years in philosophy of science (and in closely related epistemological fields) insofar as these developments focus upon three of the most important topics or problem areas of the field. Any serious listing of most crucial problem areas in philosophy of science would surely include:

(1) the nature or character of scientific theories or hypotheses—their structure and functions;
(2) the logic, or rationale of validation, of scientific theories or hypotheses;
(3) the nature of meaning and its bearing on scientific theories.[7]

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[5] The strategy of the following sections will be to indicate that his interpretation suffers, if not a credibility gap, at least a Ghibelline Guelph.

[6] In this respect, many of the works cited in these footnotes may be helpful: many of the works listed themselves contain extensive and useful bibliographies. (Indeed, even a moderately careful perusal of several of the works Miller mentions in his footnotes 3, 8, 14, and 24 would supply the reader with enough information to raise doubts about the character of his construal.)

[7] I have intentionally indicated this problem or topic under what is perhaps its broadest rubric; for I mean to include not only the concern, which such positivists as Carnap had, with the distinction between cognitive and noncognitive meaning or statements and pseudo statements, not only the Popperian "demarcation problem" of the distinction between science and metaphysics, but also the deep philosophical underpinnings of those concerns which are to be found in the development of semantics and philosophy of language from Frege through Quine. For philosophy of science—and particularly for the philosophy of social science—the searching inquiries into the nature of such central formal or semantical concepts as *reference, sense, truth, satisfaction* (i.e., of a condition or of a variable), *extension, intension, direct* and *indirect discourse, the relation of logical consequence, proof theory, model theory, axiomatization* (and other modes of formalization), have been inquiries of fundamental importance. The results of Frege, Russell, Tarski, Carnap, Wittgenstein, and Quine in semantics and philosophy of language kept pace with, stimulated, and helped form important developments in major aspects of philosophy of science over, approximately, the past fifty years. During the past decades the importance of such inquiries is represented most strikingly, perhaps, by the role of Quine's work—from that relatively early influential critique of empiricism (by an empiricist), "Two Dogmas of Empiricism" (see this and other of his essays in the philosophy of language reprinted in his *From a Logical Point of View* [Cambridge: Harvard University Press, 1953]), to his brilliant exploration of the nature of meaning and translation in *Word and Object* (New York: John Wiley and

I list these as separate problem areas for the sake of convenience. Patently, considerations falling under any one of these rubrics impinge upon some significant considerations falling under any of the others (indeed, we shall be concerned to trace some of their close connections below). In any case, they will serve well enough as touchstones in the assessment of Miller's analysis. I intend to show, with respect to each of them in turn, that Miller's interpretation of the development of philosophy of science is dubious.

As we have emphasized, the pivot of Miller's argument is that the situation in contemporary philosophy of science is a consequence of a revolutionary attack upon *positivism* by *historicism.*[8] But this is a mistaken view of current philosophy of science and epistemology. On the contrary, the changes taking place in philosophy of science over the past fifty years are more cogently to be construed as the evolution of logical empiricism under its *own* (internal) impetus rather than under impetus imparted by the attacks of an external and diametrically opposed tradition like historicism. The motivating dynamism of that evolution was (first of all) the impact of a continuing *internal* critique by the people whose work was precisely what constituted the movement in recent philosophy. I am referring, of course, to the ongoing internal critique incorporated in the works of such philosophers as Bergmann, Carnap, Feigl, Frank, Hempel, Neurath, Popper, Reichenbach, Schlick, and Waismann. (I know that not all of these are to be numbered among the "founders"—but they are well-enough credentialed to be considered among the "basic" or progenitor members.) The evolution of logical empiricism has been stimulated (second) by the impact of some more recent critiques advanced by people who were either the disciples or else, and perhaps better, the figurative children (even, in a few cases the figurative grandchildren) of these philosophical papas. I refer here to the impact of the works of such people as Peter Achinstein, May Brodbeck, Noam Chomsky, Donald Davidson, Nelson Goodman, Adolf Grünbaum, Olaf Helmer, Richard Jeffrey, John Kemeny, Grover Maxwell, Hilary Putnam, W. V. Quine, Wesley Salmon, Israel Scheffler, Dudley Shapere, Patrick Suppes, Morton White, and at least a score of others (none of whom can even remotely be considered an historicist). And (third) the evolution of logical empiricism has been stimulated by the impact of a group of important philosophers, who for some significant portion of their working lives in epistemology, or philosophy of science or philosophy of language, were, or are (and many of whose students or teachers were or are) in close relations of mutual influence with logical empiricists. I have in mind here people like Max Black, R. B. Braithwaite, Roderick Chisholm. C. W. Churchman, M. Cohen, John Dewey, Mary Hesse, Keith Lehrer, Isaac Levi, C. I. Lewis, Sidney Morgenbesser, Charles Morris, Ernest Nagel, F. P. Ramsey, Bertrand Russell, Kenneth Schaffner, Wilfrid Sellers, E. A. Singer, Jr., Ludwig Wittgenstein (surely of the *Tractatus*) and at least a score more of these—again, none of whom can even remotely be considered an historicist.[9] (Hereaf-

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Sons, Inc., 1960), especially Chs. 1 and 2, and his subsequent elaborations of those explorations in *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969). I will advert at least briefly to the importance of some of the aspects of this work of Quine's to philosophy of social science—to problems about "relativism" and "objectivity" in social science—in the text below. For the present, I wish simply to point out that treatment of Quine and Quine's contributions to philosophy of science during the past few years, though by any informed standard a major aspect of recent developments in philosophy of science, is omitted from Miller's account of what has been going on in the field.

[8] "By 1950 positivism was virtually dead as a philosophical movement. It had come under strong attack even in the philosophy of science. The leading theory of knowledge by this time was one, i.e., historicism, whose foundations lie in the work of Kant and Hegel or, more precisely, in the radicalization of the Hegelian tradition which occurred in the latter half of the nineteenth century" (Miller, "Positivism, Historicism, and Political Inquiry," p. 796). Incidentally, as is abundantly clear from several of the immediately succeeding sentences, Miller's reference to the "radicalization of the Hegelian tradition" is to epistemological and ontological, not to political, radicalization.

[9] I have deliberately listed Dewey as a "participant collaborator" in the evolution of what, I am arguing, is an ongoing empiricist (i.e., nonhistoricist) tradition in philosophy of science, because the "mutual influence" of which I speak is certainly documentable in this case, and because I think it is a mistake (one of Miller's mistakes of *commission*) to attempt to assimilate Dewey to historicism or to a nonobjectivist or irrationalist position in epistemology. For similar reasons, I have refrained from listing here the members of a very interesting recently developing *historicalist* (note, not historicist) "school" of philosophers and historians of science. I am referring to people like N. R. Hanson, Thomas Kuhn, Paul Feyerabend, Margaret Masterman, and Stephen Toulmin. The analysis which Miller suggests of the specific import of work like Kuhn's is also, I believe, a mistake of commission and I *shall* be adverting to it below.

See, in any case, the appropriate items cited in these footnotes for works of many of those just listed. These works bear out both the nonhistoricist positions of their authors and their contributions to the mainstream of the nonhistoricist development of contemporary philosophy of science. Not that the "mainstream" neglected, long ago, to consider or even embrace and then abandon theses which Miller attributes ter, I shall often refer to this "extended family" of philosophers, responsible for or closely associated with the "standard position" in philosophy of science as *SP philosophers.*)

In brief, I submit that the field has been, and today continues to be, largely *constituted* of the work of the individuals listed above (or that of their teachers or students). Thus, for anyone to omit mention of them or of their work in giving what purports to be an account of the current status of the field or of the field's "ascendant" viewpoints, is to give a grotesquely distorted account.

Of course, there is nothing sacred about the mere naming of names (at least outside of theology) and, in any case, Miller does name a few (see his footnote 8). He might, then, have

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to historicism. The collections comprised in Ayer (A. J. Ayer, *Logical Positivism* [Glencoe, Illinois: Free Press, 1959]), and Schilpp (P. A. Schilpp, ed., *The Library of Living Philosophers,* Vol. XI, *The Philosophy of Rudolf Carnap* [LaSalle, Illinois: Open Court, 1963]) are of special interest, for they show how early in the history of logical empiricism a "dialectical" opposition on e.g., "the given" was *internally* of special interest. The former gives some of the original papers showing how, for example in the arguments between Neurath and Schlick (35–40 years ago!), the problems of sense-data statements, protocol statements, confirmations, and direct and indirect observation statements, were given searching scrutiny.

The conflicting positions, and the rigor as well as the vigor with which they were pressed, had a profound influence on some of Carnap's most important works. Carnap had started from a position, in the 1920s, significantly different from both Neurath's and Schlick's (see R. Carnap, "Autobiographical Essay," in Schilpp, *The Philosophy of Rudolf Carnap*). By the time he had produced the influential "Testability and Meaning," *Philosophy of Science,* 3 (1926) and 4 (1937), he had arrived at a still different position under the influence of both Neurath and Schlick, and of others, as well as under the influence of his critical reflections.

Many of the essays in the Schilpp collection—particularly those by Charles Morris, Robert Cohen, Philipp Frank, Feigl, Ayer, and, above all, Carnap's autobiographical essay, together with his reply to his critics—copiously document the continuing development of the "standard position" in philosophy of science through stages which saw its positivist early fashioners themselves considering, themselves adopting, themselves criticizing, and themselves abandoning, both of the types of position that Miller sees as the protagonists in a conflict between positivists and "victorious" historicists. In particular, the substance of *both* the positions as he sketches them (pp. 798–801) have long been abandoned by the SP ("standard position") in favor of considerably more sophisticated views. I have in mind, moreover, the more sophisticated views about all three of the "points" of conflict he mentions (pp. 800–801).

For example, regarding the notion that "the data of sensation are present to consciousness in their original or pure form" (ms. p. 800), SP philosophers have long abandoned such "metaphysical uses" of terms like 'sensation' in favor of interpreting them as technical terms of psychology. I know of no SP philosopher who would deny the well-evidenced finding in psychology, anthropology, biology, and common experience concerning ways in which sensation might be affected by cultural and, indeed, by innate—that is to say by genetic, including phylogenetic, conditions. (As early as 1925 Carnap had abandoned as untenable the notion of given "simple sense-data" and had done so explicitly "under the influence or the Gestalt psychology of Wertheimer and Köhler (Schilpp, *The Philosophy of Rudolf Carnap,* p. 16).) Of course, the conclusions of historicism concerning the metaphysical import of this for the possibility of gaining knowledge of the "real world" were likewise rejected by SP philosophers.

Similar considerations apply to the view that our beliefs in any one epoch or culture are not affected by the cultural milieu in which they are set. I know of no SP philosopher who rejects the social-scientifically, well-evidenced thesis that such conditioning of our beliefs does take place. But, again, I know of none who would accept the metaphysical position which Miller indicates as the historicist metaphysical alternative, namely, that all knowledge "arises not so much from discovery of the real character of nature as from social or individual creation" (p. 800). Indeed, few, if any, SP philosophers would accept the dualism of the quotation at all—cleaving instead to the view that if there is anything which can properly be called "the real character of nature," then "social or individual creation" are certainly a part of it, not excrescences divorced from nature.

Finally, with respect to Miller's third point of contradiction between positivism and historicism, the position of SP philosophers surely has long been a rejection of both the positions Miller sketches on "the relativity of truth" (p. 800). They surely reject the notion that "knowledge that is true or 'objective' " is gained in some sense "by grasping nature as it really is." They reject the Platonic metaphor of "grasping" entirely—just as they reject the metaphor of truth as consisting of "the congruence" between "thoughts and things." Yet, the "relativity of truth" that they do accept is certainly not that of the historicist alternative. It is precisely the relativity of the semantical conceptions, fashioned in the 'thirties and before, by Tarski and Carnap. That is to say, 'truth' is construed as a predicate or term applicable to *accounts* of the world, i.e., applicable to *sentences.* But the concept of sentence *is* language-relative—a sentence may only be a sentence *of* some language or other. So, too, 'true' (and its cognates) is language-relative, and the full concept to be defined is always 'true in L' where L is some specific language.

The implications of this result of Tarski's have been immensely important in philosophy of language, in formal science, and in philosophy of science generally—but they lend no support whatever to the historicist views that Miller describes.

To sum up: the main point of this extended footnote has been to challenge the plausibility of Miller's view that the current situation in philosophy of science finds historicism holding the field after mounting a successful revolution against positivism on three specific "points of contradiction." In contrast, I have been suggesting that there has been, and continues to be, an *evolution* of a prevailing or standard position in the field, fashioned by the ongoing *internal* critique of such nonhistoricist philosophers of science as those named above.

Of course neither 'revolution' nor 'evolution' is the given an alternative account, a less misleading one, by simply confining his attention to a careful treatment of the actual substantive developments which have taken place in fifty years respecting some of the main problem areas in the field. Had he done so, some better perspective on the actual role, if any, of historicism might have been gained. But here, too, the essay has omitted the review of most of the problems which are at the center of attention in the field. Such a review would surely have resulted in a more adequate account than has the rather provincial focus that appears to have informed the essay.[10]

The Nature of Scientific Theories

As a case in point, the evolution of the standard position in philosophy of science, under the impact of a relentless but *internal* critique, is perhaps most perfectly illustrated by the problem of the nature of theory in science. As in other fields, so too in philosophy of science,

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clearest term to have come down the ideological pike. The continuing rebuilding of the ship of philosophy of science while it is under weigh (to change metaphors and borrow the figure from Neurath which has been used so effectively by Quine) has yielded a remarkably changed vessel. So, if one wants to call the craft, now afloat, a revolutionary rather than an evolutionary outcome, why, it would be pointless to quarrel. Whatever the epithet, the one thing that *is* clear is that the craft is still manned by the extended crew which has been working it over for decades and that crew is not historicist.

[10] This provincialism is sharply in evidence in his discussion of the "historicalist school" (which could, as well, be called "the history-of-science-school") of philosophers of science (ms. p. 804). Here, Miller fastens upon the interesting views, critical of both the method and some of the substantive results that have been put forward by *SP* philosophers. The *history-of-science-school* is a convenient label for a group of writers, of whom N. R. Hanson (*Patterns of Discovery*. Cambridge: Cambridge University Press, 1958), Paul Feyerabend ("Problems of Empiricism" in R. Colodny, ed., *Beyond the Edge of Certainty*, Englewood Cliffs, New Jersey: Prentice-Hall, 1965), Stephen Toulmin (*The Philosophy of Science*, London: Hutchinson, 1953; and *Foresight and Understanding*, London: Hutchinson, 1961), and Thomas Kuhn have been especially influential. Kuhn's *The Structure of Scientific Revolutions* (Vol. II, No. 2 of Otto Neurath, ed., *The International Encyclopedia of Unified Science*. Chicago: University of Chicago Press; a second edition of Kuhn's book revised and enlarged by a new chapter entitled "Postscript 1969" published as a Phoenix Paperback, Chicago: Chicago University Press, 1970), in particular, has been widely read and commented on. (The book has gained currency outside "the field". In numbers, diversity, and perhaps acumen, its heterogeneous reading public appears to have outstripped considerably the reading public of many of the essays on it—including some containing Kuhn's own later criticisms and modifications of his theses.)

Miller's appraisal of the signifiance of this line of development and these works, particularly Kuhn's contributions, is unconvincingly narrow on at least three counts. First, Kuhn's arguments about the nature of scientific revolutions are focused on just one problem area in a field which has a large number of other significant problem areas occupying attention. Even if Miller were correct in his apparent appraisal of the cogency of Kuhn's views, no case whatever has been made (and it would have to be a strong and detailed one to carry much weight) that prevailing views or positions in these *other* areas had been overturned or replaced.

In the second place, Miller is unduly narrow in not taking account of the critical treatment, and the cogency of that treatment, which Kuhn's views (and the views of other of the "historicalists") have received and which have tended to vitiate the impact of those views even within their circumscribed problem areas. A wider and deeper account of the literature (in, say, the last ten years) would surely temper the notion that in confrontation with such arguments as Kuhn has put forward, the standard position has been shattered and superseded by an historicist position. For a discussion of the weaknesses occurring at many of the pivotal junctures in Kuhn's arguments (and other historical arguments) see, for example, the discussions by Watkins, Toulmin, Popper, Lakatos, and indeed, Kuhn himself, in *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave, (Cambridge: Cambridge University Press, 1970). Dudley Shapere's "The Structure of Scientific Revolutions," *Philosophical Review*, 73 (1964), 383–394 and "Meaning and Scientific Change" in *Mind and Cosmos*, ed. R. Colodny (Pittsburgh: Pittsburgh University Press, 1966), cited but not expounded by Miller, provide careful assessments of Kuhn's arguments which show them to be uncompelling. The doubts Shapere raises about crucial aspects of the "historicalist" arguments have still not been resolved by them. If, though, there is the opportunity for only one detailed assessment of the ramifications of positions like Kuhn's and positions like historicism for the concept of *objectivity* in science, I would urge careful attention to Israel Scheffler's book, *Science and Subjectivity* (Indianapolis: The Bobbs Merrill Co., Inc., 1967). It contains an incisive and meticulous analysis of what some of the better articulated arguments about objectivity in science come to. They are evaluated lucidly and their defects, where they occur, are exhibited in a way which enhances our understanding both of the nature of claims for and against objectivity in science, and also of some of the problems about objectivity which remain as challenges to inquirers.

A third manifestation of what I've been calling the "narrowness" or "provincialism" of Miller's perspective on the current status of philosophy of science is to be found in its simplistic assimilation of Kuhn's position to the irrationalist historicism which Miller delineates. It seems fairly clear that Kuhn himself would reject the charge as Miller formulates it. See, especially, Kuhn's discussion of the charges of irrationalism and relativism in his elegantly titled second essay, "Reflections on my Critics," in Lakatos and Musgrave, pp. 259–266. And consider by way of sample, the following remarks by Kuhn in the essay:

> "Members of a scientific community can, I am held to have claimed, believe anything they please if only they will first decide what they agree about and then enforce it both on their colleagues and on nature. The factors which determine what they do choose to believe are fundamentally irrational, matters of

its journals and "Series" volumes perused over a period of time provide a fair clue to the topics which most exercise the profession and also to the way in which work on such topics develops.

In philosophy of science, one especially important series (rightly termed an "excellent" one by Miller) is the *Minnesota Studies in the Philosophy of Science*. Under the general editorship of Herbert Feigl, Michael Scriven, and Grover Maxwell, at least five volumes have been published since 1956. This series furnishes a good historical insight into what have been major foci of attention in recent times. The series is certainly dominated by SP philosophers, but its contributors also number several authors (e.g., Feyerabend) whom Miller identifies as rebelling against the received or standard position.[11] In its fifteen-year span, though, the se-

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accident and personal taste. Neither logic nor observation nor good reason is implicated in theory-choice. Whatever scientific truth may be, it is through-and-through relativistic.

"*These are all damaging misinterpretations*. . . ." (p. 260) (emphasis added).

"To name persuasion as the scientist's recourse is not to suggest that there are not many good reasons for choosing one theory rather than another. It is emphatically *not* [emphasis in original] my view that 'adoption of a new scientific theory is an intuitive or mystical affair, a matter for psychological description rather than logical or methodological codification.' On the contrary, the chapter of my *Scientific Revolutions* from which the preceding quotation was abstracted explicitly denies 'that new paradigms triumph ultimately through some mystical aesthetic,' and the pages which precede that denial contain a preliminary codification of good reasons for theory choice. *These are, furthermore, reasons of exactly the kind standard in philosophy of science:* accuracy, scope, simplicity, fruitfulness, and the like" (p. 261) (emphasis added).

". . . no part of the argument here or in my book implies that scientists may choose any theory they like so long as they agree in their choice and thereafter enforce it. Most of the puzzles of normal science *are directly presented by nature* [emphasis added], and all involve nature indirectly. Though different solutions have been received as valid at different times, nature cannot be forced into an arbitrary set of conceptual boxes. On the contrary, the history of proto-science shows that normal science is possible only with very special boxes, and the history of developed science shows that nature will not indefinitely be confined in any set which scientists have constructed so far" (p. 263).

". . . I have not previously and do not now understand quite what my critics mean when they employ terms like 'irrational' and 'irrationality' to characterize my views. These labels seem to me mere shibboleths, barriers to a joint enterprise whether conversation or research. My difficulties in understanding are, however, even clearer and more acute when these terms are used not to criticize my position but in its defense. Obviously there is much in the last part of Feyerabend's paper with which I agree, *but to describe the argument as a defense of irrationality in science seems to me not only absurd but vaguely obscene*. [emphasis added] I would describe it, together with my own, as an attempt to show that existing theories of rationality are not quite right and that we must readjust or change them to explain why science works as it does. *To suppose, instead, that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process* [emphasis added] is to open the door to cloud-cuckoo land" (pp. 263–264).

These quotations merely indicate *Kuhn's* view of the ascription of *irrationalism* to his view of science—and, of course, Kuhn may be wrong about the import of his own views. *Prima facie*, however, they call for a stronger case than Miller has thus far provided for assimilation of Kuhn to the irrational historicism that he describes.

To sum up: it may be a too narrow perspective which leads Miller to claim, without sufficient warrant, (1) that the cogency of a critique of a standard position on one problem area would shatter the standard positions taken over the entire field of philosophy of science; (2) that the critique *is* cogent and has had that effect even in its "own" problem area; and (3) that the basis of the critique can be assimilated to an irrationalist historicism which he delineates.

[11] Paul Feyerabend comes closer, perhaps, than any other of the philosophers, referred to by Miller (and others, e.g., Shapere) as being "in revolt" against the "standard view," to maintaining the "irrationalist-relativist" position which Miller describes as historicism. Neither the precise import of Feyerabend's remarks in this regard, nor what he intends is always clear. (Sometimes his essays have the character rather of performances than of arguments—this would by no means, incidentally, be regarded as a pejorative ascription by Feyerabend. See, e.g., his "Against Method: Outline of an Anarchistic Theory of Knowledge" in H. Feigl, M. Scriven, and G. Maxwell, general eds., *Minnesota Studies in the Philosophy of Science* [Minneapolis: University of Minnesota Press], Vol. 4, ed. M. Radner & S. Winokur [1970], "Philosophy of Science, A Subject with a Great Past" in *Minnesota Studies* . . . , Vol. 5 [1970], and "Consolations for the Specialist" in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*.) Feyerabend has contributed essays (some very long ones, in fact) to several volumes in the *Minnesota Studies* series and to other of the "establishment" series in philosophy of science. Despite his coming closer to being a "Miller-historicist" than anyone else of significant influence presently among the historicalists, it would again be a distortion to assimilate even him to the irrationalist-relativist model—it would be a mistake to equate his passionately expressed commitment to pluralism, the "principle of proliferation" (see "Against Method," p. 26 and *passim*)—simplistically with irrationalism. One key to the actual complexity of Feyerabend's views is to be found in his alignment of them, explicitly, with J. S. Mill's reasoned defense of pluralism of intellectual, artistic endeavors and life styles *within* any society which hopes to avoid the suppression of truth or happiness. What may have evaded Miller's attention is that the kind of irrationalist, relativist historicism *he* is describing would be epitomized by successive "monolithic" world outlooks—each coordinated to the epoch or society or culture to which it is relative. But such a view, I suggest, may be incompat-

ries has been remarkable not only for providing a setting for vigorous critiques of the standard view (which, like Feyerabend's, are made by people who are relatively far out from the center of the mainstream) but also for exhibiting, in fine anatomical detail, how the center itself shifts under the impetus of its own self-scrutiny. A clear-cut specimen of this latter phenomenon culminates in C. G. Hempel's essay in the 1970 volume (Volume 4 of the series), *Analyses of Theories and Methods of Physics and Psychology*. In his essay "On the 'Standard Conception' of Scientific Theories," Hempel provides a characteristically lucid account of problems which beset the provision of an adequate analysis of the structure of scientific theories. His discussion is searchingly critical of the prevailing view on scientific theories (which, of course, he, as much as any other philosopher, has helped to fashion—hence, it is a searchingly self-critical discussion) and it is suggestive of significant changes which should be made in that view. Briefly, in the standard view, then, scientific theories are construed as (1) sets of sentences or statements (in any case, *linguistic entities*) which are (2) systematically related, (3) include some law-like generalizations (i.e., statements asserting nomologically regular connections among events of various kinds), and (4) furnish an empirically testable account of a thereby empirically determinable subject matter.

Now, each of the numbered parts of the characterization just given is the locus of a persistent syndrome of problems whose elaboration and resolution have pretty much *constituted* philosophy of science in this particular problem area for the past fifty years. In terms of the present case, however, a summary of the "establishment's" own conception of theories changing under the impetus of an internal critique (given in this example by Hempel),[12] is primarily related to clauses (2) and (4). The following informal account of the evolution is given here in moderate, but perhaps sufficient, detail to show that Miller's account of the character and course of this position is deficient.

Since at least Norman Campbell's *Physics*, the kind of *systematic* relationship taken to hold among a theory's statements has been a *deductive* relationship. At a very early stage of

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ible with (or at any rate, quite different from) the methodological pluralism Feyerabend espouses. For the latter is to be a pluralism *within any one* epoch or society or culture. One hardly can see J. S. Mill as the intellectual or stylistic hero of Millerian-historicism; and Feyerabend's enthusiasm for Mill should make us suspicious of any facile categorization.

I believe that Feyerabend's most important contributions have been the additions that his astute and lively historical analyses (of concrete instances of the history of theory-change and theory-choice) have often made to the long continuing body of work constituting an *internal critique* (from at least Neurath forward) of the "observation-theory" distinction in philosophy of science. I do not think that close attention to his specific arguments about concrete cases— often very illuminatingly presented—would warrant the conclusion that he eschews rationality, considerations of evidence, scope, power, simplicity, openness to test, to criticism, and many other of the quite traditional touchstones of scientific appraisals of theories.

The burden then of this note, as was the burden of the two preceding ones, is to suggest that Miller's vision of a *revolution* in the enterprise is a dubious vision: for, among other reasons, the group of philosophers of science he fastens on, by no means uniformly, individually or collectively, hold views than can be easily equated with *his* historicism. Moreover, the major views held by this group have by no means come to dominate contemporary philosophy of science or (I should say 'and *a fortiori*') philosophy at all—even if their views were to be found compelling, their thrust or comprehensiveness is by no means coextensive with the whole of philosophy of science. And finally, their views, or at least major aspects of them, and the vehicles they choose for their expression of them (e.g., note that T. S. Kuhn's *The Structure of Scientific Revolutions* is Vol. II, No. 2 of the *International Encyclopedia of Unified Science*) are more plausibly to be construed as evolutionary changes, or attempts which, if successful, would constitute further evolution of, rather than revolutions against, the evolving mainstream in the last 50 years.

[12] Hempel himself, in a footnote ("On the 'Standard Conception' of Scientific Theories," in *Minnesota Studies*, Vol. 4, p. 146) provides a list of works which, in recent times, represents "characteristic stages in the evolution" of the "standard" construal of scientific theories, and I cite the list he gives here: N. R. Campbell, *Physics: The Elements* (Cambridge: Cambridge University Press, 1920), chapter 5; F. P. Ramsey, "Theories" (in Ramsay, *The Foundations of Mathematics and other Logical Essays*. London: Routledge and Keegan Paul, 1931); R. Carnap, *Foundations of Logic and Mathematics* (Vol. I, No. 3 of *The International Encyclopedia of Unified Science*. Chicago: University of Chicago Press, 1939), especially sections 21–25; R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1953), Chapters I-III; R. Carnap, "The Methodological Character of Theoretical Concepts," *Minnesota Studies*, Vol. 1, ed. H. Feigl and M. Scriven (1956), pp. 38–76; R. Carnap, *Philosophical Foundations of Physics*, ed. M. Gardner (New York: Basic Books, 1966), part V; E. Nagel, *The Structure of Science* (New York: Harcourt Brace & World, 1961), chapters 5 and 6.

Hempel is, however, too modest inasmuch as he has omitted two important landmarks in the evolving path of the standard construal: his *Fundamentals of Concept Formation in the Empirical Sciences* (Vol. II, No. 7 of *The International Encyclopedia of Unified Science*, 1952) and his "The Theoretician's Dilemma: A Study in the Logic Theory Construction" in *Minnesota Studies*, Vol. 2, ed. H. Feigl & G. Maxwell (1958). The brief sketch of the standard position's evolution I give is wholly indebted to the list just cited and numerous other works too: as indicated above; the sketch entails, perforce, very severe simplifications.

logical empiricism[13] the notion of a formalized language with observational *primitives* in which the scientific theory, in axiomatized form, was embedded or embeddable, was a fairly widely discussed starting point. Some set of logically strongest generalizations comprises the axioms of the theory; the statements composing the theory are the derivable theorems which follow deductively from the axioms.

The formal *systematic relatedness* of a theory's statements is thus construable as that, at least potentially, of an axiomatic systematization. Just as, in standard formulations, axiom systems are divisible into "unproved" statements (the antecedently explicitly specified axioms) and "provable" statements or theorems derivable as logical consequences, so, too, the system's *terms* are divisible into a set of *primitives* (i.e., the terms *undefined* in the system) and a set of *defined* terms—the *remainder* of the theory's vocabulary—which is defined *by* the set of primitives. This means that, in principle, *all* the defined terms of a theory thus formalized are *dispensable* in favor of the primitives of the theory; i.e., anything which may be "said" at all in the theory may be said by definitionally replacing each expression containing defined terms by some expression containing only its definitionally equivalent primitive expressions. In this sense, definitions are rules of elimination.

If the entire theory may thus be couched in the vocabulary of its primitive terms, then the selection of a set of primitives for the formalized system becomes a critical matter in several respects. For example, in any *interpreted* system (one which has an explicitly characterized *semantic* dimension in the form, say—following Carnap—of semantical *rules of designation* for its primitive terms and *rules of truth* for sentences in which nondesignative expressions appear), the choice of primitives thus explicated will determine the meanings of every sentential context of the theory. In the literature of the subject, criteria for the choice of a primitive basis for a system almost invariably include the antecedent *clarity* of the primitives.[14] This seems a plausible enough requirement, since all defined terms of the theory are defined *by* primitives, and since what may be asserted by the sentences of the theory *is just what can be asserted by its sentences couched in primitive notation* (or else the eliminability condition would be violated). It seemed, indeed, a *sine qua non* of "knowing what one is talking about" in employing the theory that the primitive terms be antecedently clear.

It is at this juncture that the traditional "demands" of empiricism are given weight. In the fictionalized "childhood" of logical positivism being here recounted, it is held that those words referring to the direct objects of sensations, are those whose meanings are clear—one knows what such terms refer to. All other terms are *prima facie* suspect. The rigorously austere empiricist must earn the right to use such "suspicious" terms by showing that they are eliminable—by showing that he doesn't *have to* use them—by showing that they are definable in (antecedently clear) terms that refer to what can be directly sensed.

The fulfillment of these traditional empiricist requirements ostensibly dictates, as choice of primitive terms for scientific theories, just those terms which refer to what may be directly sensed or observed.[15] Any new concept or term being introduced into the theory must then be definable on the basis of the primitives.[16] Since the orientation of this primeval positivism was also to be tested by its adequacy as a rational reconstruction of science, however, the set of primitives chosen would have, in fact, to fulfill at least a double requirement: they would, as we have seen, have to refer to directly observable features of the world in order to satisfy the traditional empiricist demands; but they would also have to be a "rich enough" or a "powerful enough" set of primitives to furnish an *adequate basis* for the already under way, *prima facie* "established," scientific theories. These two constraints upon the choice of primitives are recognized to be in tension. The pull of the first is for keeping the primitive basis *narrowed* to just the observation terms which we are "surest of" in the sense of being antecedently the most clear in their application. The pull of the second constraint is toward *widening* the basis—toward being sure to admit at least as many primitives as constitute a set rich enough for the full demands of established science. This latter pull has been stronger and has tended to liberalize the criteria for what might be admitted as a primitive term over the full period of the past fifty years. Thus, such criteria, as well as concomitant conceptions of the structure of scientific theories itself, have continued to be changed under the ongoing internal critique of positivists who (like Neurath) never found the narrowest aspects of the program at all palatable.[17]

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[13] So early, in fact, as to be mythological. I know of no one in the history of logical empiricism (certainly no one in the last 40 years—and, indeed, not even Hume) who actually held the view being outlined. It seems to have been used often (as is being done above) as a didactic or expository device which (perhaps like the "contract theory" of the formation of polities) allows for brief (and dangerous) shortcuts to the illumination of more complex issues. (For one version, of such an idealized account, which bears on the social sciences see R. Rudner, *Philosophy of Social Science* [Englewood Cliffs: Prentice-Hall, 1966], chapter 2.)

[14] For an important and illuminating discussion of criteria for, and problems of, selecting primitive bases, see N. Goodman, *The Structure of Appearance* (Cambridge, Mass.: Harvard University Press, 1951), chapter 3.

[15] I am deliberately disregarding any differentiation here between 'sensing' and 'observing' or between 'data of direct sensation' and 'observed data'. Such differences come to be important, however, in the ensuing historic discussions.

[16] Only thus could another part of the program attributed to this mythological, primeval stage of logical positivism be carried out: the "strong verificationist" tenet to the effect that an hypothesis or a theory be itself logically implied by the set of *observation* consequences which verified it.

There were at least two important stages of the resulting "liberalization." The first was a *loosening* of the requirement that new concepts or terms, ostensibly not referring to directly observable features of the world, could be introduced into a systematized theory only by definition on the basis of primitive observables. The new "looser" requirement came to allow *nondefinitional reduction* of an introduced concept to the observational basis.[18] These "reductions" were accomplished by coordinating the term to be thus introduced with two or more observational terms which were formally related to it in one or more *reduction sentences.* (For the most part, such terms would be *dispositional* terms such as 'attitude', 'belief', 'habit', 'soluble', 'magnetic', 'flexible', 'inflammable', 'playable', etc.—none of which, it was held, refers to directly observable features of the world, and for which no *generally* satisfactory *definitional* mode of introduction seemed available.)

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[17] Carnap's views in particular were gradually modified during the 'twenties and early 'thirties under the impact of Neurath's critique and the results of his own continued deep explorations into logical problems of theory and formal language construction. See, in particular, his accounts in Schilpp, *The Philosophy of Rudolf Carnap,* pp. 16–24, 863–865, 882–886; for the influence of philosophers, other than Neurath, with whom Carnap held discussions (including, incidentally, many philosophers such as Charles Morris, Ernest Nagel, Sidney Hook, and C. I. Lewis who were in the pragmatist tradition) see, e.g., Schilpp, pp. 29–36, 40, and 860–862. Now, as Carnap points out (Schilpp, p. 50), under the influence of such philosophers as Mach and Russell, he very early (at the time of *Der Logische Aufbau der Welt* (Berlin: Weltkreist-Verlag, 1928), pursued the quest for certainty believing then that "the task of philosophy consists in reducing all knowledge to a basis of certainty." However, he also goes on to add that in "the Vienna [Circle] discussions my attitude changed gradually. . . ." Moreover, it is true that certain traditional empiricist doctrines influenced Carnap to believe that this "basis of certainty" was to be found in the "immediately given" data of sensation. Yet, to identify the general view itself as empiricism is again to be unduly simplistic: such an identification ignores, on the one hand, the fact that the tradition of the quest for certainty (as an attempt at "reducing all knowledge to a basis of certainty") has its main roots in Descartes and Leibnitz (not to say Plato!); on the other hand, it ignores the strong elements of corrosive scepticism in such empiricists as Hume—and, indeed, ignores a long antecedent tradition of empirioscepticism back to classical times. In any event, that quest for certainty, along with any notion of the epistemological *primacy* of phenomenalist bases *was early abandoned.* In the 'thirties and before, even Carnap was holding that such a phenomenalist claim was "meaningless metaphysics." Despite the early and repeated published demurrers, however, it has always been easier for superficial readers of the ongoing tradition to identify positivism with the vulnerable "childhood" position of just some of its proponents, rather than to make instead the arduous effort required to understand the technically complex ways in which those positions were evolving in even the early days. Apropos of this, there is a touch of rueful amusement in Carnap's account (Schilpp, p. 38) of a conversation with Einstein at Princeton (*c.* 1952).

> "On one occasion Einstein said that he wished to raise an objection against positivism concerning the question of the reality of the physical world. I said that there was no real difference between our views on this question. But he insisted that he had to make an important point here. Then he criticized the view, going back to Ernst Mach, that the sense data are the only reality, or more generally, any view which presumes something as an absolutely certain basis of all knowledge. I explained that we had abandoned these earlier positivistic views, that we did no longer believe in a 'rockbottom basis of knowledge'; and I mentioned Neurath's simile that our task is to reconstruct the ship while it is floating on the ocean. He emphatically agreed with this metaphor and this view. But then he added that, if positivism were now liberalized to such an extent, there would no longer be any difference between our conception and any other philosophical view. I said that there was indeed no basic difference between our conception and his and other scientists' in general, even though they often formulate it in the language of realism; but that there was still an important difference between our view and that of those traditional philosophical schools which look for an absolute knowledge."

In the light of the last sentence one is again struck by how odd will seem a contrast which has not been very severely and precisely qualified, between an *absolutist positivism* on the one hand and a *relativist historicism* on the other. In its light, also, aspects of the stance that Miller takes toward the contrast become implausible to anyone closely familiar with the development and present situation of positivism or, better, with the development of the standard view in philosophy of science.

[18] The device of the *reduction sentence* is introduced explicitly by Carnap in 1936 in "Testability and Meaning." Under the impact of criticisms of this "way out" by Goodman (*Fact, Fiction and Forecast,* 2nd edition, New York: The Bobbs-Merrill Co., Inc., 1965), Scheffler (*The Anatomy of Inquiry,* New York: Alfred Knopf, 1963), Hempel ("The Theoreticians Dilemma"), Pap, and others, Carnap came ultimately to modify his position on reduction—in a direction that might be fairly characterized as even greater liberalization. See his "The Methodological Character of Theoretical Terms" (1956) and "The Philosopher Replies" (in Schilpp, *The Philosophy of Rudolf Carnap,* pp. 958–966). Note that neither the 1936 liberalization or the subsequent ones, though of considerable importance in the evolution of the "standard view," can properly be called "historicist changes" or "historicist-caused changes."

The second liberalization is one only in relationship to the simplified account of the "primeval" program given above and, of course, is *not* chronologically a subsequent stage. In actual fact, the much more complex view of the structure of scientific theories which Norman Campbell had described by 1920[19] was already, at that time, well understood and accepted independently[20] by philosophers who were associated with the Vienna Circle. This more complex version of scientific theories in more and more refined expositions came to represent the "standard conception." Again, note that these refinements or changes have been produced under internal critiques such as those already mentioned above, until, by the time Hempel is writing fifty years later in a 1970 volume (his essay "The 'Standard Conception' of Scientific Theories" in Volume 4 of the *Minnesota Studies* series), we have a fairly precisely characterizable and influential "orthodoxy." It is an "orthodoxy" which can, in that same volume, be taken account of both by Herbert Feigl, in giving an exposition complete with a diagrammatic outline (Feigl, "The 'Orthodox' View of Theories: Remarks in Defense as Well as Critique"), and also by Hempel who, in his essay, signals the continuing vitality of the evolving "establishment" by again proposing (*non-historicist*) significant improvements or changes in that standard conception.

Before discussing the import of Hempel's criticism and proposed changes, we may very briefly note some ways in which the standard conception actually differs from the "mythical" one given earlier. In the first place, and most important, the standard conception does *not* require definitional eliminability of the *theoretical* (that is, the most abstruse and "remote-from-observation") terms or concepts. These theoretical terms themselves occur as *nonobservational* primitives in the postulates, axioms, or *purely theoretical* sentences of the theory. Moreover, the primitive, theoretical vocabulary is not interpreted by semantic rules of designation. What "meaning" such theoretical terms have, it is held, is conferred in two ways: (1) It is conferred by the "implicit definition" imposed upon them by the formal syntactical structures and relations among the theoretical sentences (postulates or axioms) in which they appear. This "syntax" places certain limitations of a formal nature on the kind of interpretations or meanings that *could be* consistently given to such concepts—but the syntactical properties by no means uniquely determine such interpretations. (2) Meaning is also conferred by the addition to the theoretical sentences of the theory of a second set of sentences. This second set contains, also, some observational terms, thus setting some observational conditions for the applicability or use of the former sentences to which they are logically related, and at least partially thereby determining their "reference" or "usage" or "meaning."

This more ramified conception, then, views scientific theories as consisting of at least the following: (a) a *theoretical vocabulary,* V_t (in theories of physics such "nonobservables" as 'electron', 'positron', 'electronic orbit'; in social science theories, perhaps 'group morale' might turn out to be such a term; in Jungian psychology, 'collective unconscious'); (b) an *observational vocabulary,* V_o; (c) a set of *theoretical sentences* whose (extralogical) terms are made up wholly out of items in V_t; these theoretical sentences are systematized axiomatically and, by virtue of the uninterpreted character of their vocabulary, thus constitute an uninterpreted system or *calculus;* (d) a set of *bridging sentences* containing terms of both V_t and V_o which function to confer some "empirical import or applicability" upon the calculus.[21]

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[19] See Feigl, "The 'Orthodox' View of Theories: Remarks in Defense as well as Critique" in *Minnesota Studies,* Vol. 4 (1970), p. 3 and early works by both Carnap and Schlick cited by Feigl.

[20] Feigl, "The 'Orthodox' View of Theories . . . ," p. 6. This exposition and critique is put forward as a common basis for the discussion of the contributors to the volume who are focusing upon aspects of the standard view.

[21] These "bridging sentences" (as Hempel now calls them) or their counterparts have been variously described in the literature as "dictionary sentences," "operational definitions," "coordination rules," "correspondance rules." One of Hempel's contributions to the field has been his perspicuous elaboration of this conception of theories over a period of decades. His formulation and discussions of this conception in *Fundamentals of Concept Formation in Empirical Science,* "The Theoreticians Dilemma," and *Aspects of Scientific Explanation* (New York: The Free Press, 1965), together with those of Braithwaite in *Scientific Explanation* and Nagel in *The Structure of Science,* are, perhaps, the most widely known and influential formulations both inside and outside the field. One of Hempel's conclusions has been that the sentences of a theory—in particular, its theoretical sentences—cannot be considered to be meaningful or true or false in iso-

It should be noted that thus developed the standard position represents a considerable weakening of the first of the two constraints, the "traditional empiricist demand," mentioned above, in favor of the second—the "adequacy-to-established-science" constraint. And it should, perhaps, again be emphasized that this "looser" position or conception—going back as it does at least to Campbell, Carnap, and Schlick in the 'twenties, is not only *not* a consequence of historicist attacks on positivism, it is not even a consequence of *historicalist* attacks (see footnote 9, 10 and 11 for an indication of the difference between these two types of attack). In any case, it is against *this* standard conception (already "empirically weakened" by contrast to the mythical, primal conception) that Hempel directs his latest criticisms and proposals for modification. Two of the major targets of his criticisms are, first, the standard conception's assessment of the importance of axiomatizing the theoretical sentences of a theory, and second, retention of a classification of a theory's terms into a theoretical vocabulary and an observational vocabulary.

With respect to the first of these, Hempel points out that fulfillment of the requirement for axiomatization *per se* will offer no advance in clarity and will provide few, if any, of the benefits that the requirement was supposed to confer. Among other reasons for this judgment, he points out that, generally speaking, many alternative axiomatizations will always be possible in the formulation of a theory—and that each of these alternatives may have properties which make them significantly different. The injunction to axiomatize, accordingly, is unhelpful unless it is accompanied by a specification of "some particular and appropriate kind" of axiomatization (Hempel, "The Standard Conception," p. 152). For, "whatever philosophical illumination may be obtainable by presenting a theory in axiomatized form" will depend upon the fulfillment of substantive criteria over and above, or independent of, criteria fulfilled by its merely being an axiomatization (Hempel, p. 152).

Respecting the second major criticism, Hempel has come to believe that the notions of an "observational term" and the related notion of "empirical meaningfulness" have significant deficiencies as used in elucidating the standard conception of theories. He has come to this view not only as a result of his own reflections but very largely under the influence of Quine's critique[22] of the analytic-synthetic distinction and the related presuppositions about meaning under whose spell logical empiricism had long tried to explicate the notion of empirical import *vs.* theoretical import.

Hempel's positive proposal is that the whole conception of *specifying the meaning of new theoretical terms on the basis of old ones* should be abandoned, since it depends upon what Quine and others have shown to be a defective view of the nature of "meaning." He tells us to acknowledge that the proper applicability and correct use of both the new and the old terms (the "theoretical" and the "antecedent" or "pretheoretical" vocabularies, as he calls them) will depend very often, not upon such specifications, but upon nondefinitional or nonstipulational and presumably extrasystemic modes of learning to understand terms. The standard conception of theories presupposes "that we cannot come to understand terms except by way of sentences specifying their meanings with the help of previously understood terms." Surely, Hempel continues (p. 163) "this notion is untenable. We come to understand new terms, we learn how to use them properly, in many ways besides definition: from instances of their use in particular contexts, from paraphrases that can make no claim to being definitions, and so forth."

These relatively bare indications of the current status of this one problem area—the structure of scientific theories—should suggest that Miller's claim of a revolutionary overthrow of positivism by historicism is neither a *fait accompli* nor an impending possibility.[23]

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lation from other sentences of the theory—that the *unit of meaningfulness* or "cognitive significance" may best be construed as the theory or theoretical system *as a whole*. (See, e.g., "Empiricist Criteria of Cognitive Significance" in *Aspects of Scientific Explanation*, p. 117.) In this regard, Carnap appears to have continued to differ with Hempel (see Carnap's "The Philosopher Replies" in Schilpp, *The Philosophy of Rudolf Carnap*, pp. 961–966); but, in any case, the position Hempel has now come to is a departure *both* from his older, *and* from Carnap's, versions of the standard conception.

[22] The Quinian notions (see W. V. Quine's *Word and Object* [N.Y.: John Wiley, 1960]; and *Ontological Relativity and Other Essays* [N.Y.: Columbia U. Press, 1969]) which support this sort of further modification by Hempel of the standard view also underlie his regarding historicalist positions like those of Feyerabend and Kuhn with doubt (see *Minnesota Studies*, Vol. 4, pp. 155–156, and also in the same *Studies* volume, Hempel's remarks in the "Discussion at the Conference on Correspondance Rules," pp. 223–227, 253–254).

[23] Hempel's proposed modifications, however important, and however clearly "departures from orthodoxy" still, it will be noted, regard theories as *deductively systematized* sets of statements. The proposal does *not* eschew all axiomatization in suggesting it be "appropriate axiomatization," and while discarding the theoret-

The Validation of Theories

I can, with the above background, be briefer about the second "touchstone" problem area: *the validation of scientific hypotheses or theories.* If anything, since the historicist view purports to make some claims about "objective" and "nonobjective" validation of knowledge, omission from Miller's essay of any account of the main developments or the current state of this area is even more startling. For what the essay has omitted is pretty much everything that has been going on: for example, the whole course of the development of probability theories and theories of validation generally. His essay fails to consider, or even mention, the monumental work of people associated with early stages of the Vienna circle—the work of Carnap and Hempel on logical theories of confirmation, of Popper on corroboration, of Reichenbach and Von Mises on frequentist theories of probability, of Ramsey on the foundations of subjective probability, of Nagel in his comprehensive and searching critique of extant theories and his suggestive treatment of a Peircean view. Nor does Miller consider or allude to the significant, more recent developments in statistical probability, statistical decision theory, and personal or subjective probability; likewise he fails to consider or mention the critical and constructive influential work of Goodman on a theory of projection or the work of Churchman, Braithwaite, and others on the relationship of the making of value judgments to choices of rules or policies for acceptance or rejection of statistical hypotheses. Neither these nor similar developments are considered in an essay which purports to be bringing news of the current state of the field.

What his essay gives us instead are allusions to just one, the Kuhnian-historicalist one, of the many interesting strands of development in this area. But I have already suggested (see, for example, footnotes 10 and 11) that Miller's account of even this one "strand" commits vitiating errors: neither his claims about the "field-overwhelming" revolutionary character, nor the irrationalist historicist character, nor even the tenability, of, for example, Kuhn's main views on the validation of theories, have plausibility.

The Nature of Meaning

The third and last touchstone problem-area I am employing in criticising the adequacy of Miller's account has to do with the nature of meaning and its bearing upon scientific theories. In this problem area, too, serious omissions simply weight his paper misleadingly as an account of the recent or current status of the field.

Perhaps the two most striking omissions are the essay's failure to attend to the ramifications of the contributions of Quine and of Chomsky (and, of course, of the work of the many able people who have been elaborating or criticizing those contributions). This is an especially unfortunate omission in the case of Quine—not only because of the centrality of Quine's contributions in the field, but because of the two recent "sources" that are explicitly mentioned—Wittgenstein and Kuhn. Indeed, Kuhn acknowledges that his theses draw significantly on some of Quine's results.[24]

In some ways it is difficult to assess the impact of the later Wittgenstein (Miller's second "source") on philosophy of science—both because it is difficult to assess the precise import of his doctrines generally and also because very little work in philosophy of science explicitly lays claims to late Wittgensteinian influences.[25] His work has, however, had obvious importance in the philosophy of language—at least,

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ical-observational dichotomy among terms, his proposal replaces it with a "new theoretical" *vs.* "antecedently available" term dichotomy. A close perusal of the essay will, in fact, further bear out that it is much more a continuation of Hempel's development of the standard view than it is a radical or revolutionary (or, for that matter, in any sense a *historicist*) total break with that developing tradition.

[24] Kuhn, himself, may have come somewhat belatedly to an increasing recognition of the bearing of Quine's results upon his (Kuhn's) arguments about the nature of what is involved in coming to accept a new theory. See especially his "Reflections on my Critics" in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge,* pp. 266–271 and 275–277, for a discussion of Quine's results and of "the utility, indeed the importance of the linguistic parallel" (p. 267) to problems of theory change in the history and philosophy of science.

Incidentally, I am refraining from anything more than the reminder of the importance of Chomsky's brilliant and original work which is conveyed simply by the mention of it. For that work has had fairly wide currency outside of philosophy. Perhaps, though, it would be suitable just to remark that though Chomsky's work is, clearly, one of the developments of significance in the problem area, and though it is, in an interesting sense *antiempiricist,* it is by no means irrationalist or historicist; it is by no means antiscientific. Miller gives this work no consideration.

[25] Peter Winch's *The Idea of a Social Science* (New York: Humanities Press, 1958), is an interesting exception—at least as far as his explicit claim to derive from Wittgenstein is concerned. On the other hand, it's not at all clear that that claim is justified—there are some grounds for believing Winch to have misconstrued Wittgenstein—in particular in his contention (a sort of "social-privacy" view that might well have been rejected by Wittgenstein) that speakers of a language are the only *possible* adequate observers of the *linguistic behavior* of those speakers.

if we reckon the sheer number of first-rate philosophers who are attending to it.

Yet, whatever the importance of Wittgenstein for philosophy generally, clearly an account which focused only upon his contributions to the present status of problems of meaning—even an account focused just upon him as an influencer of the historicalists—would give a misleading picture. For, it would (as Miller's essay does) exclude the contributions of Quine and Chomsky. If the essay's failure to note the contributions of philosophers like Chomsky and Quine,[26] who have produced significant work in the problem-area of "meaning," may be considered an error of omission, the claim that Wittgenstein and Kuhn are progenitors of, or proponents of, an irrationalist-historicist position on meaning is surely an error of commission.

The fact is, that Kuhn and Wittgenstein make rather strange philosophical bedfellows. In the first place, they have incompatible views about philosophical methodology. This, by itself, suggests that they are not likely to hold or generate the same philosophical positions. Thus, for example, Kuhn does *not* eschew rational reconstruction. In his "Reflections on my Critics" (in *Criticism and the Growth of Knowledge*, edited by I. Lakatos and A. Musgrave, Cambridge University Press, 1970), he tells us on pp. 236–237.

> I am no less concerned with rational reconstruction, with the discovery of essentials, than are the philosophers of science. My objective, too, is understanding of science, of the reasons for its special efficacy, of the cognitive status of its theories . . . my criterion for emphasizing any particular aspect of scientific behavior is . . . not simply that it occurs, nor merely that it occurs frequently, but rather that it fits a theory of scientific knowledge.

But rational reconstructionism (as is well documented in even the sources that Miller cites) was anathema to the later Wittgenstein.

Of course, the incompatibility of Wittgenstein's and Kuhn's views on the nature of philosophical method (while showing that they could not both, consistently, be irrationalist-historicists) does not entail that either embraces historicism in any form. To adduce passages showing that the later Wittgenstein believed that "linguistic activities are part of a total cultural tradition, or 'form of life'" scarcely serves to distinguish Wittgenstein from any philosopher of science who has even a nodding acquaintance with anthropology. To go on to claim, as Miller does, that Wittgenstein is a historicist because he believes that it is impossible to give "a theoretical account of speech," is to mistake a *general* Wittgensteinian precept about philosophical method for a specific view on the concept of *"Lebensform."* Again, the fact is that Wittgenstein believed that *philosophy* could not cogently give "theoretical accounts" of *any* concept—i.e., he believed that it was not the business of philosophy (in contrast to science) to give theoretical accounts, theoretical analyses, rational reconstructions, etc. of anything at all, *hence, a fortiori, not of a Lebensform or speech or any linguistic entity.* But Wittgenstein did not deny that science could give a scientific account of a lifestyle or of speech. He did hold that philosophy should not give theoretical accounts—that it should proceed, as he does in the *Philosophical Investigations* by showing, *not* telling, what is or might be wrong when we are puzzled philosophically. This view of appropriate philosophical methodology was not, in short, restricted to philosophical problems about speech, and there is nothing in the view which implies a historicist or an irrationalist position about science.

In any case, Wittgenstein's view, taken as a thesis about "the problem of theory-change," would remain unconvincing to mentors of the standard position in philosophy. An objection, similar to the one Hempel makes (in volume 4 of the *Minnesota Studies*) against the historicalist contentions on the incommensurability of theories, could be made against the construal of Wittgenstein that Miller appears to subscribe to. Just as the historicalist position on meaning is wholly vulnerable to the charge that *it* furnishes no adequate criterion of sameness of, or identity of, meaning, so this construal of Wittgenstein would be vulnerable to the charge that *it* furnishes no criteria for judging sameness of, or identity of, *lifestyles.* To sum up, with respect to our third touchstone too, Miller fails to give even the outlines of the much more inclusive account of men or works in this area that he would really need to give in order to bulwark a thesis about the recent and present status of philosophy of science. At the same time, the account of the philosophers he does give, commits crucial mistakes respecting the import or implications of their views.

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[26] In speaking explicitly of the omission of consideration of Quine and Chomsky, I do not, of course, mean to suggest that Miller's account is guiltless of other important omissions as well. A particular case in point, again, is Goodman's important work on criteria for accuracy and adequacy of definitional, or explicational systems (see *The Structure of Appearance*, chapters 1–3), as well as his work bearing upon problems of choices of hypotheses (e.g., in *Fact, Fiction and Forecast*, chapters 3 and 4). Of great importance in this problem area, too, has been the work of such philosophers (not alluded to in Miller's account) as Wilfrid Sellars, Gustav Bergmann, J. L. Austin, Bertrand Russell, Rudolf Carnap, Alonzo Church, Alfred Tarski, Donald Davidson, Israel Scheffler and many others who have contributed to continuing work on the concepts of meaning and reference and upon the implications of these concepts for problems in philosophy of science.

"Positivism" and "Behavioralism"

In the preceding sections I have tried to indicate that Miller's "news" of a successful revolution in philosophy of science needs to be taken with a grain of salt. My task in those sections was fundamentally historical: I attempted to show (1) that there is a lot more going on in philosophy of science than is to be found in the bailiwicks of Wittgenstein or the "historicalist" schools; (2) that in philosophy of science, changes in what still remains the "established viewpoint" have been evolving continually under the impact of *internal* critiques—that is, that the overwhelming preponderance of changes have come, and continue to come, from inside the "establishment;" (3) that there are good reasons for being dubious about Miller's assimilation of either the "historicalist school" or Wittgenstein to the position *he* characterized as "historicist;" (4) that pivotal views of "historicalist school" philosophers, such as T. S. Kuhn, far from "defeating" the prevailing views, have been sharply and effectively criticized (for example, by I. Scheffler, D. Shapere, and others) and, apparently, are continuing to be modified by their originators under the impact of such criticisms; (5) that in the light of points (1) through (4) talk of a "victory" by "historicists" in philosophy of science (and hence in epistemology generally) is rather wrongheaded.

So much, then, for historical quarreling.

My task in this section bears more directly on a *substantive* issue in philosophy of science rather widely misunderstood outside of the field—one, indeed, which is still the subject of controversy within the field. The substantive issue is, just what kind of philosophical underpinning does the position of "behavioralism" in science—and in the present context specifically, behavioralism in political science—require for its foundation (or, as Miller puts it, for its "epistemological roots"). The corollary issue which connects this more closely with Miller's essay is, "Is behavioralism so connected, logically, with positivism that the rejection or refutation of the latter renders the former not cogent?"

Miller's claim that behavioralism "has its epistemological roots in positivism" must be examined closely. For, whether behavioralism has, *logically*, its epistemological roots in positivism is quite independent of whether the espousal of behavioralism in any given instance was *caused* by "positivists" (or, for that matter, by "antipositivists"). The *causal* claim is a historical one essentially, the logical claim, a problem of the logical analysis of the positions of behavioralism and of positivist epistemology. I will suggest that the *logical* connection Miller's views seem to require does not, in fact, hold. One last preliminary point: in inquiring whether behavioralism depends logically on positivism in such a fashion that a refutation of the latter position implies a refutation of the former position, it should be noted that no judgment need be made regarding the cogency of either position. The inquiry into their connections may be consummated without finding either position compelling.

In the previous sections I tried to show that Miller's characterization of positivism was, historically, misleading. In this section, I want, first, to set out some of the defects of his substantive account of positivism. But finally I shall attempt to show that even were we to accept his account—however much of a caricature it might be—the problem of whether the position so characterized has the relationship to *behavioralism* which Miller has imputed to it would still yield a negative resolution.

Now, the tasks as just circumscribed would be relatively simple if Miller had provided an explicit characterization of the behavioralist position (even a caricature of it) as he has of the "positivist" or standard positions. Unfortunately, he has not. Still, Miller does cite, with apparent concurrence, the descriptions of behavioralism (however partial) given by certain political scientists (John Gunnell, Sheldon Wolin, Marvin Surkin, and Henry Kariel) in antibehavioralist discussions. These citations help us to infer what Miller takes behavioralism to be.

The following characterizations of behavioralism are cited (and apparently accepted) by Miller:

(1) (John Gunnell) Behavioralism adopts a "deductive model of explanation."

(2) (Sheldon Wolin) ". . . a belief in the primacy of method, resembling the methodological emphasis of Descartes, is the distinguishing feature of the behavioral movement."

(3) (Miller) "His [Marvin Surkin's] methodology is fundamentally different from behavioralism in this respect: Recognizing its own contingent and ideological character, it dedicates itself to criticizing and changing the existing society rather than to preserving it."

(4) (Miller, paraphrasing Henry Kariel) "Posi-

> tivistic political science [presumably a synonym for *behavioralist political science*], in particular, had identified the real with the observable, discredited speculation about alternative possibilities, and reconciled man to his present fate."

This is a scant and disappointing harvest.[27] Still, we must ask ourselves whether the position of behavioralism thus delineated presupposes the version of the "positivist epistemology" which Miller explicitly presents. In the essay's version, we are told that

(a) ". . . the critical task for a positivist epistemology is to uphold the assumption that our generalizations about the external world are reliable if and only if they are constructed from or tested by the raw material of experience."

(b) "Accordingly, positivists have typically argued that perception makes us directly aware of something 'given,' something which has not yet been affected by our judgments."

(c) "Furthermore, these given data are taken (individually or in combination) to represent the facts of the external world. Positivists . . . would largely agree that we directly perceive something, some *particulars,* prior to our conceptualization and reasoning."

(d) "This epistemological viewpoint asserts further than the human mind, when it operates normally or naturally, perceives the same thing in the same way regardless of time and place. It is for this reason that knowledge based on experience has an 'objectivity' that no other claims to knowledge can possess."

(e) "In order to be meaningful, our statements about the external world must be tested against the data of experience."

This group of statements yields what I have described above as a caricature of the positivist position. Before passing judgment on what logical connections it can be said to have with the scant characterization of behavioralism Miller presents, a closer look at each of its five points (a)-(e) may be helpful.

Note that (a) is defective on at least two counts. First, its use of the biconditional ('if and only if') is a logical howler. It has the surely unwanted consequence of claiming that positivists hold experiential testability to be a *sufficient* condition for the reliability of a generalization about the external world, hence that every such generalization *and also* its contradictory or its contrary are reliable. No positivist ever held any view remotely like this—at most, positivists have held experiential testability to be a *necessary* condition for "reliability." Moreover, in the light of the very early acceptance of the standard conception (e.g., Campbell's and Carnap's 'twenties conceptions, or Hempel's 1952 summary) only theories *as wholes,* not single (theoretical) generalizations, are taken to be testable; and a generalization's experiential testability is not even a necessary condition for its reliability (or, for that matter, for its meaningfulness—Miller's point (e) to the contrary not withstanding).

Second, the last phrase of (a) appears to claim that positivists adopted a phenomenalist epistemology. To be sure, Miller goes on to say that positivists may not, after all (or, at least not all of them) really be adopting a phenomenalist epistemology. Yet I think Miller does trade on his original suggestion. For, the suggested alternative to it, i.e., the positivist adoption of the "corrigible" language of *physicalist* (in Miller's terms "common sense") objects and events, renders much less plausible the claim made here and in (b) and (c) and, possibly, (d). The suggested claim is that positivists hold perceptual judgments, or just normal perceptions to be unaffected by social conditioning, genetic (including evolutionary) endowment or other circumstances of the social or "natural" environment. But it is surely well known to positivists (who are not the least scientifically *au courant* of philosophers), that the findings of psychologists, anthropologists, biologists, geneticists, and other scientists, provide strong evidence for the rejection of the hypothesis that "perceptions" are unconditioned by factors or circumstances of the social and natural environment. It would be surprising if positivists were not persuaded of this hypothesis on the basis of the evidence put forward by researchers in their several fields. But more than this, it would be wholly out of character and wildly at variance with their pro-science, traditionally antimetaphysics stance, for positivists to deny (presumably in some access of metaphysical fervor) that scientists *could* find evidence that would bear on the hypothesis about perceptions one way or another; and it is just such an extraordinary denial with which Miller is saddling the positivist epistemology. In any case, the issue of what implications for the notion of objectivity such extraordinary imputations to positivism have, and what implications the contrary imputations to historicism have, is one which may be joined in the context of Miller's subsequent points.

Let us then consider Miller's points (b), (c), and (d) which, roughly, hang together. In the light of what has been argued just above, (b) presents, again, some of the puzzles we noted in (a) concerning the positivists' stance toward unconditioned perception. On the other hand, (d), or at least the imputation concerning the human mind in its first sentence, has strongly the flavor of a tautological imputation. Presumably not even Miller's historicists would claim that what is causally efficacious, in the conditioning of changing perceptions—or in the changing of the "human mind"—is merely and *simpliciter* a change of date or a change of location. Presumably only something that is different *on* that changed date or *at* that changed location may affect perceptions—for the historicists in Miller's ken this will be, presumably, some sociocultural difference. So the imputation embodied in the first sentence of (d) seems so tautologously, or truistically, innocuous that it could be accepted by both positivists *and* historicists—indeed, by virtually any epistemological position.

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[27] I wonder if David B. Truman, who gave a characteristically judicious (wholly unfrenzied) welcome to behavioralism in his 1955 Brookings Lecture, "The Impact on Political Science of the Revolution in the Behavioral Sciences," would recognize the "baby" from the description we've garnered here.

Perhaps, then, the bone of contention between positivist and historicist can best be unearthed in (c). As a description of positivist epistemology, though, (c) quite misses even the historical, not to say the present-day, target. Thus, when the positivists took the "linguistic turn"—an orientation which, surely, characterizes its earliest stages—they did so in part just out of realization that formulations like the claim in the first sentence of (d) are otiose. "Perceptual data" for positivists (as for anyone else, no doubt) are just the perceptual data that they are. Deciding the question of whether any such data "represent the facts of the external world" becomes, on the linguistic turn, simply the question of deciding whether a hypothesis, something like 'Jones was not hallucinating when he had a perception of X', is to be accepted or rejected. The question of whether some perceptual data "do represent the facts of the external world" (and positivists *do* believe that some people, some times, do hallucinate) is, *if not a scientific question* like the above, then for positivists, traditionally, *not a question at all.*[28]

The second sentence of (c) presents us with a more complicated claim. In addition to the part of its import that we have already noted, it attributes to positivist epistemology the view that our perceptions of particular things are "direct" and that they are "prior to our conceptualization and reasoning." Unless one concluded this was just a repetition of the charge about unconditioned perceptions already discussed above, one might think, alternatively, that this claims that positivists do not believe that we can have *indirect* perception of something. Is this really true? We might ask, under what circumstance the hypothesis, 'Jones indirectly perceived X' would be accpeted by positivists. Taken literally, as we have just interpreted it, this part of Miller's claim suggests that there are none. But clearly, if a positivist, noting a familiar shadow through a translucent window were to claim, 'I perceived Jones', the question of whether to apply the terms 'directly perceived' or 'indirectly perceived' would be settled by positivist and historicist in the same way—i.e., without any nontrivial differences on usage. That is, both could agree on usage without in the least compromising any epistemological commitment or difference that either held. For example, both positions might agree to withhold the predicate 'directly perceives Jones' (in the sense of 'sees Jones') unless an appropriate and describable irradiation pattern of retinal receptors had taken place—so that *seeing Jones's shadow* would *not* fulfill this necessary condition.

Thus, the occurrence of 'directly' in Miller's point (c) appears to carry no special force or weight. Of course, if he intended only to say in another way what he had said above, namely, that to hold that we "directly perceive" is to hold that we perceive unconditioned by social or other environmental or genetic circumstances, then the occurrence of 'directly' has already been dealt with in the considerations advanced above.

The last aspect of (c) requiring attention is its claim about *priority*. Miller charges positivist epistemology with the view that in perceiving, we have the perception of *"something,* some particulars, prior to our conceptualization or reasoning." Part of the difficulty here is an understanding whether the claim here is about logical priority or about physical (i.e., temporal) priority. If the claim is a logical one to the effect (that positivists hold) that perceptions necessarily first occur in some pure, unconditioned way and that the conditioning (i.e., conceptualizing) step is a distinguishable step, then our remarks above about positivists accepting scientific hypotheses about perception being a conditioned process, would apply here too. On the other hand, Miller might here be ascribing to positivist epistemology the view that, chronologically, perceptions (presumably unconditioned) occur first and these are *subsequently* conditioned by being considered in some conceptual framework or in inferential relationships.

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[28] It is, in the rhetoric of damnation from early, or biblical, positivism, a pseudoquestion.

I think, in the light of the preceding discussion, two things might be remarked of this latter view. First, it is quite compatible with our traditional positivism, delineated above, to hold the view that some perceptions occur earlier than the specific conceptualizations under whose conceptual framework they may come subsequently to be considered. Second, traditional positivists could hold such a view about the *temporal* priority of perceptions, without inconsistency even though they accepted the universal hypothesis that no perceptions occur unconditioned. For, they might compatibly with their traditional positivist principles, accept not only that hypothesis, but also the hypothesis that no conceptualizations and no inferences occur unconditioned by social or physical environment or genetic endowment. But however veridical it may be, this is not the view of positivist epistemology that Miller appears to require for his argument.

His fifth point has been already extensively treated and the shortcomings of the claim it embodies about the positivist view of meaning have been discussed.[29]

The upshot of all of these considerations is not simply to cast doubt upon the historical adequacy of Miller's characterization of positivist epistemology—that ought, after all, to have been accomplished by the discussion in the preceding sections—but also to show, if the considerations adduced are cogent, that his characterization, because of its logical or conceptual inadequacies, will not do one of the main jobs he requires of it. He wants it to highlight the *contradictions* between positivist and historicist epistemologies. But these contrasts or contradictions are vitiated or blurred by the conceptual difficulties that mar the five-pointed earlier presentation.

Thus, the substantive remarks that are made in the essay about the nature of positivism appear also to be defective. And now, finally we may turn our attention to the specific claim that the "death" of positivism entails the "death of behavioralism because behavioralism has its roots in positivist epistemology." In one sense, the question is moot, for the drift of our argument has been to deny that positivism—at least in the sense of the continually developing standard position in philosophy of science—is moribund. But we have also promised an assessment of a claim that does not depend on any position on the actual death of positivism. It is the claim that positivist epistemology, even as delineated by Miller, and behavioralism are logically so related that the refutation of the former would entail the refutation of the latter.

It should be clear that if, (1), we waive the critique just given of the five points and take the original position in its pristine articulation; if (2) we refrain from trying to derive any more rigorous reconstruction of the behavioralism based on Miller's gleanings from authors critical of behavioralism; and, moreover, if (3) we instead allow our intimate familiarity with behavioralism to substitute, as now seems appropriate, for analysis of Miller's scanty portrayal; then we can give the claim of logical connection between behavioralism and positivism relatively short shrift. The answer is that behavioralism certainly is not logically connected with the position delineated in (a) to (e)—though it might be with some reconstructed account that our analysis of (a) to (e) may have indicated. In particular, note that behavioralism need only hold that observations of political behavior may be relevant to the testing of hypotheses about political behavior. Behavioralism implies no position whatever about the "pure given" or about *unconditioned perceptions*. Should no perceptions be unconditioned, this would in *no way* show that the scientific study of political behavior was impossible, or was even made more difficult.

Again, behavioralists, like physicists, need not forego the use of very general or abstract hypotheses even when those hypotheses are not testable in isolation from other theoretical statements. *For it is not at all incompatible with behavioralism to hold that it is whole theories, rather than isolated hypotheses,* whose "meaningfulness" may be assessed in science. Theoretical statements which may not be "testable by experience" may still acquire meaning derivatively if they are parts of a theory which, *as a whole,* is so testable. We may conclude, then, that a refutation of Miller's caricatured positivism would *not* require the abandonment of the scientific study of political behavior—that is, that behavioralism does not presuppose *his* positivism.

The arguments against behavioralism which Miller adduces in particular from just the battery of antibehavioralist political scientists,[30] seem similarly uncompelling. For example, insofar as the behavioralists adopt the current standard position on scientific explanation, they are by no means restricted to the deductive model of scientific explanation. The deductive model was relatively simple to elucidate and, perhaps as a consequence, enjoyed a considerable currency among philosophers and scientists. But the SP philosophers (see especially Hempel's review and discussion in volume 1 of the *Minnesota* series) have long held the view that explanations do *not* require to be cast deductively; and these philosophers have provided *inductive* nomological theories. Miller quotes Wolin on a behavioralist tenet which Wolin calls "the primacy of method" and which he sees as "resembling" Descartes' methodological emphasis. I think that the comparison ought much more plausibly be made to another believer in the primacy, or "supremacy" of method—John Dewey. It is surely clear from Dewey's work that any behavioralist can reject Descartes' quest for certainty and still devote his working life fully to the rational employment of scientific method.

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[29] Still we should, at least in a footnote, point out that Miller must surely in (e) intend 'test*able*', rather than 'test*ed*'—otherwise the view he *intends* to portray would be too far removed to be even called a caricature of positivism.

[30] I am attending here to all but the third characterization of behavioralism (3), from Surkin. Surkin seems to be berating behavioralism, or positivism, for expressing the wrong ideology or, at any rate, for giving aid and comfort to, and generally tending to work at "pre-

Kariel's cited contributions to the scanty look at behavioralism also fails to show the vulnerability of behavioralism. Kariel is simply mistaken in thinking that behavioralism needs to identify "the real with the observable," but he may be right about the way behavioralists themselves *actually* behave (even though their behavioralism does not entail that they do so). The point is, that whatever the alliances or political activities in society are, that behavioralists manifest, they are not *forced* to such alliances or activities by the "logic" of behavioralism.

My conclusion, as indicated, is that the "demise" even of Miller's version of positivism does *not* entail the "demise" of behavioralism; moreover, nothing in the characterization of behavioralism given by the authors he cites supports Miller's view to the contrary. Of course, a main point of the analysis given just above is that the kind of claim that Miller makes (about logical connections that may hold between epistemological and methodological positions), requires a much more ramified and rigorous presentation of support than his essay delivers. Beyond this, I should like to suggest that the behavioralist view— insofar as it is a methodological view rather than a substantive one in political science—is, in fact, logically *independent* of a very great variety of the epistemological stances which have been taken in the history of philosophy. If, for example, the central methodological tenet of behavioralism in political science is that political behavior be adducible as evidence relevant to the acceptance or rejection of political hypotheses, then, first, it is hard to see how this is incompatible with any of the usual flavors of epistemology that have been historically available to us—including "positivism" *and* "historicism"; second, it is clear that none of the four characterizations "leaned on" by Miller and brought together above is a constitutive characteristic of behavioralism.

Behavioralists need not (and the more *au courant* do not) restrict science to deductive models of explanation. Belief in the primacy of method, in the sense of a judgment of the relative moral importance of using canons of science to investigate, over the moral importance of abandoning scientific inquiry in favor of nonscientific political action, is also *not* a constituent of behavioralist belief. Behavioralism would be compatible with either moral stance. Finally, Kariel's charge, apparently accepted by Miller, that behavioralism identifies "the real with the observable" is simply not accurate. In short, it appears to be somewhat premature to abandon behavioralism—certainly premature if that abandonment depends for its grounds upon any recent philosophical developments which are alleged to necessitate such a change.

In summary: (1) I have argued that Miller is mistaken in construing the present or recent situation in philosophy of science or epistemology as one in which a conflict has raged between positivism and historicism. (2) I have argued that he is mistaken in believing that such a conflict has ended with a revolutionary victory of irrationalist historicism over positions that have become standard ones in philosophy of science. (3) I have argued that Miller's account of what is happening in the field is a distorted and misleading view in that it misconstrues the substantive views of positivist and other SP philosophers. I have argued that Miller's essay does not show itself to be fully abreast of the detailed criticisms that have been given of the philosophers (like Kuhn and Feyerabend) whom it seems to champion as the purveyors of an allegedly ascendant historicism. (4) Finally, I have tried to show why I find uncompelling Miller's claim about a substantive connection between his version of "positivist epistemology" and behavioralism in political science.

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serving" the existing society. It may be the case that, as a matter of fact, behavioralists have manifested this "conservative" political cast—but they surely need not, on the basis of their *methodological* positions, have done so; they surely are not barred by their position from *pursuing scientifically* whatever problems the anti-behavioralists pursue.

Comment: On Objectivity

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While I thoroughly enjoyed reading Miller's paper—it is a very graceful presentation, I am nevertheless left with a sense of disquiet. Its strokes are so broad and its statements so sweeping, that it tends to gloss over a series of rather critical problems. And given its enormous range of coverage, it becomes a virtual impossibility, in the space allowed me, to respond to each of the elements that I consider problematic.

One could, for example, raise question as to the imputed relationship between logical positivism and behavioralism. When the overwhelming emphasis that logical positivists have placed on formalized logics is taken account of, "it is fair to say" that their influence on the behavioral movement is a matter to be investigated. There has been, of course, much discussion of science in general and, more particularly, of formalization and axiomatization by political scientists, but it is rather obvious that the bulk of behavioral research has exemplified a "barefoot empiricism." It is only recently, with the advent of mathematics into political science, that one observes a movement toward formal modes of explanation. Unless numbers are equated with logical positivism, the line between it and behavioral research becomes rather tenuous. It was Graham Wallas who urged, in 1908, that we must learn to think in terms of statistical curves, and the National Conference on the Science of Politics in the 1920s was responding, not to the Vienna Circle, but to the extensive use of "tests and measurements" during World War I; and the upsurge of survey research in the 1950s owed more to advances in research design and statistical technique than it did to Schlick, Carnap, Feigl and Hempel. It is also of interest that many "behavioralists"—even today, resist the "logical reconstruction" of formal models as arid and sterile, as for too distant from experience. The case could be made that the political scientists who fashioned behavioralism vastly overestimated Bacon.

Or, one wonders why there is so much discussion of phenomenology—and of what is here selected to represent phenomenological researches—when the only studies that bear even remotely on politics are those of Schutz, Garfinkel, and company. I have no doubt that we shall soon see some rather interesting researches along these lines and I have no hesitation in suggesting that these will be strictly empirical—i.e., if they follow Schutz and Garfinkel.

Or, how does it happen that the new "leading theory of knowledge" arises at a time when an intense power struggle is going on in the profession? Here, I should have assumed that a paper which takes its cue from Mannheim and the historicity of the mind would have introduced a more specific consideration of the relation between ideological and epistemological claims. There is more to the relativist thrust than contemporary disputes in the philosophy of science.

But above all, what I find most disturbing is the lack of any sustained analysis of relativism and objectivity in a paper which is certainly addressed to these themes. Accordingly, I have chosen to center my remarks on these problems in the hope of stimulating a discussion that may yet lead toward a solution of these problems. Too often, it seems, we go around in circles.

My first comment offers some criticism of Miller's use of the category "historicism"; my second is a consideration of relativism—a doctrine which I think is entirely untenable; my third, a digression perhaps, is designed to correct some impressions (in political science) of the Vienna Circle; my fourth is a consideration, in detail, of the concept of objectivity; and my final comment is a brief note of caution on the use of Kuhn's study as a legitimating agent. I beg off direct comment on Gunnell, Wolin, Connolly, Kariel, Surkin and Jung because this would require another paper. But I have addressed some of their writings elsewhere.[1] Insofar as Easton is concerned, I agree that he has made errors.

(1) As a preliminary comment on "Positivism, Historicism and Political Inquiry," I wish to call attention to the rather loose construction of the term "historicism." As presented in this essay, it carries a "soft focus";—its connotation is so diffuse as to give rise inevitably to ambiguity. An admission incidentally, that historicism is an ambiguous term does not by itself help to clarify: rather, it signals the need for clarifica-

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[1] Martin Landau, *Political Theory and Political Science: Studies in the Methodology of Political Inquiry* (New York: Macmillan, 1972).

tion. For concepts of this kind are a patent liability in any domain of inquiry, especially as regards methodological problems, because they conduce to the indeterminate and the indefinite. It needs to be emphasized that the function of a definition is exactly the opposite—to make a class term, a category or concept *definite.* Terms which have loose meanings, which are vague and ambiguous, generate the elasticity of equivocation. They allow for alternative and uncertain reactions. And this means that no amount of observation can settle a dispute. Answers can never be clearer than the questions which prompt them.

Moreover, diffused connotations tend also to be "noisy." They frequently contain a confusing variety of characteristics, many of which are inconsistent with each other, many of which are unrelated to each other. The result is then the collection of a miscellany that one often refers to as an "oversimplification." (Parenthetically, I should like to distinguish this type of concept from one that is general or comprehensive: the mark of the latter is that it points to invariances—properties or relationships that seemingly diverse phenomena have in common without the clutter of incidental associations.) Oversimplification results in the clustering of so disparate a group of philosophers as are here assigned to the class "historicist," thereby permitting a bypass of such profound differences as, for example, that between historical determinism and historical relativism. A historical materialist has no difficulty in stating that the basic method of knowing is induction and that by "analysis of sense-data one can inductively reach the most general and objective . . . laws of nature."[2] Further, the soft focus in evidence permits the confusion of relativism as an epistemological position and the distinctive empirical hypothesis known as the "historicity of the mind." These are not *necessarily* connected, and one can find historical determinists (e.g., Engels and Marx) holding to the latter as they reject the former. One can even find Mannheim separating epistemological relativism from the empirical hypothesis that we have no choice but to plagiarize the *Zeitgeist*—i.e., that social time, place, and circumstance condition our thought. This separation is not at all unusual: on the contrary, it must be made. And the reason, as I shall show below, is this: If the principle of epistemological relativism is correct, then the thesis of the historicity of the mind cannot be accepted as correct; it cannot even be decided upon.

As a final note on Mr. Miller's classification scheme, let me add the following: If an opposition to phenomenalism, epistemological relativism, and the historicity of the mind admit to the set (historicism), if the qualification for membership is the possession of *all* of these properties, then we shall have to curtail assignment drastically. And if the attribution of any *one* of these, if any single property admits to the set, then it shall have to be enlarged *ad infinitum.* I may very well be wrong, but the only way to make sense of so motley a collection of philosophers as Miller has assigned to the class "historicist" is to assume that they are unified by some form of rejection of phenomenalism—which would then make Otto Neurath a historicist.

These considerations bear directly on Miller's assertion that after a century of protracted conflict, historicism has vanquished positivism and has become our leading theory of knowledge. I think that this claim is patently false—despite the fact that positivism has undergone some profound changes since the days of the Vienna Circle. Nor has historicism had very much influence on the shaping of contemporary approaches. Indeed, the criticism of the "historicists" has received scant attention, only that necessary to point out that the relativistic theory is self-refuting. It would have been more germane to contemporary developments in the philosophy of science had Miller hooked the thesis of the historicity of the mind to those philosophers who reject "presuppositionless" theories (as do Hanson, Feyerabend, and Kuhn) but this is an entirely different problem, one that does not necessarily lead to a relativism that enjoins objectivity. Such philosophers stand apart from the "perpetual beginner" (Merleau-Ponty's phrase) of phenomenology who, in following Husserl's command, takes "to the things themselves" *without preconception.* For to Husserl, phenomenology was to be a rigorous, purely descriptive study, eminently scientific in the most fundamental sense—and it is toward this goal that metaphysical speculation and theoretical postulates were to be "bracketed." His quarrel with the empiricist turned on the hypothesis that "experience" was far more varied, or complicated, or richer than the latter would or could admit. But for this "historicist" it was the things themselves that were to be the final arbiters. Rejection of the logico (mathematical)-empirical model in the study of human behavior did not transform Husserl into a relativist. Relativism to him was, clearly, an epistemological mistake.[3] How then do we find him included in the set? The answer is to be seen in the soft and noisy character of a concept (Miller's "historicism") that breeds a panoramic oversimplification. And it is just such oversimplification that permits the claim that historicism has vanquished positivism to appear plausible. But we should remember that our dictionaries (e.g., *Webster's 7th New Collegiate*) instruct us that to oversimplify is to bring about distortion, misunderstanding and error.

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[2] Narcyz Lubnicki, "Epistemological Problems of Dialectical Materialism," *Synthèse,* 7 (1948), 292. "Objective" in the Leninist (materialist) sense.

(2) Miller offers a second proposition: that the recent antibehavioral movement is an expression of historicism; that it not only opposes the methodology of behavioralism but rejects its epistemology (positivist) as well. This, in fact, is what he sets out to prove.

Frankly, I should have thought this to be rather evident.

To the scholars who demand "an end to political science," to those (many at any rate) who inhabit the antitextbooks, and to the writers who have pronounced on methodism and the apolitical politics of the scientific effort, it can fairly be stated that positivism is the devil and objectivity the sin.

In the interest of clarity, we should dispense with the term "historicism" and confine our discussion to "epistemological relativism." This is the doctrine that challenges the concept of objectivity, denying outright that there can be objectively true facts. On this matter, I am not at all certain of Miller's position although he appears to share my view that the denial of objectivity is fundamentally untenable. In any case, the antibehavioralists have pressed their relativism with unrelenting vigor—employing the thesis of the historicity of the mind to demonstrate that all is ideology.

As a passing note, it may be said that this is an irony. Where, in earlier generations, the antiscientific thrust was built on the principle of volition—on free will or self-determination, the contemporary formula holds that man is so completely culture-bound, so fully determined socially, as to thoroughly preclude the attainment of objectivity. The formula, of course, is Mannheim's: "The detachment of the individual from the group," from the "matrix of which (he) thinks and experiences" is a fiction. All knowledge is "dependent upon the subjective standpoint and the social situation of the knower" and "it is impossible to conceive of absolute truth existing independently of the values and position of the subject and unrelated to the social context." The thought of all parties in all epochs, Mannheim insists, is of an ideological character.[4]

By this formulation, the social scientist is trapped—locked into a cell from which there is no exit. All assertions offered as fact are, at best, "partial truths." The *partiality* is always there. A statement, for example, on the "decline of ideology," is itself ideological, as are hypotheses about equilibrium, pluralism, and findings with respect to political behavior. These mask an ideology, conceal bias, hide valuations, and are always *of necessity* reflections of the interests of some social group or class. Even conscious awareness of these liabilities does not help—"for the truth which a mind thus socially conditioned is able to grasp is likewise socially conditioned."[5] Those who hold to the principle of objectivity, it is said, have not faced up to the implications of "Mannheim's Paradox."[6]

It is more appropriate to observe, however, that it is the relativists themselves who have not done so. They seem to be entirely unaware of the "self-referential inconsistency" of Mannheim's formulation. Almost a quarter of a century ago, Merton summed up the obvious contradiction involved: "Mannheim's conception of the general total ideology . . . leads at once, it would seem, to radical relativism with its *familiar vicious circle* in which the very propositions asserting such relativism are *ipso facto* invalid."[7] Repeatedly, philosophers have asked: How can one find out about the social determination of knowledge if even the validity of the attempt is historically conditioned? Or, if the proposition that every rationale is an ideology is itself *objectively* true, how can Mannheim know this? Or, is this proposition singularly exempt from the class of assertions to which it applies? If so, why so, and if not it is incorrect by its own formulation.[8] In short, Mannheim's proposal, like those of all epistemological relativists, takes the following form: We can never be sure of *x* because of *y*, but *y* turns out to be an instance of *x*.[9]

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[3] See: Edmund Husserl, *Cartesian Meditations* (The Hague: Nijoff, 1960), pp. 153–6, and Dorion Cairns (translator of *Cartesian Meditations*), "An Approach to Phenomenology," in *Essays in Memory of Edmund Husserl*, ed. Marvin Farber (Cambridge: Harvard University Press, 1940). For an interesting discussion of similarities between phenomenology and empiricism see also Richard Zaner, *The Way of Phenomenology* (New York: Pegasus, 1970), pp. 37–40.

[4] Karl Mannheim, *Ideology and Utopia* (New York: Harcourt, Brace and World, Harvest edition), pp. 28, 79. This discussion of objectivity and relativism is drawn from Landau, *Political Theory and Political Science*.

[5] Hans Morgenthau, "Reflections on the State of Political Science," *Review of Politics*, 17 (October, 1955), 446. Morgenthau writes,

> The mind of the political scientist is molded by the society he observes. His outlook, his intellectual interests, and his mode of thinking are determined by the civilization, the national community, and all the particular religious, political and economic and social groups of which he is a member. The 'personal equation' of the political scientist both limits and directs his scholarly pursuits. The truth which a mind thus socially conditioned is able to grasp is likewise socially conditioned. The perspective of the observer determines what can be known and how it is to be understood. In consequence, the truth of political science is of necessity a partial truth.

[6] See Joseph LaPalombara, "Decline of Ideology: A Dissent and an Interpretation," *American Political Science Review*, 60 (March, 1966); J. Peter Euben, "Political Science and Political Science" in *Power and Community*, ed. Philip Green and Sanford Levinson (New York: Vintage, 1969); Charles A. McCoy and John Playford, eds., *Apolitical Politics* (New York: Crowell, 1967), Introduction; and Marvin Surkin and Alan Wolfe, eds., *An End to Political Science* (New York: Basic Books, 1970). These three volumes contain numerous references to Mannheim's Paradox.

We have a graphic illustration of this dilemma in Miller's paper when he writes: "If any final truth is to be admitted, it is the principle that no final truths about the nature of things are possible." Apart from the question of internal consistency, it is not at all evident why this claim should be granted a privileged status. As Nagel would put it, if there is one such claim, why not others? And the sentence which follows the quotation reads, "There is no possibility of a supra-temporal truth which can be grasped from above the historical stream." By the logic of the relativists, this "objective truth" is clearly invalid. Or as Ben Walter puts the whole business:

> S-1: All empirical propositions about social life are (a) perspectively conditioned, and (b) therefore lack objectivity.
>
> But is it also the case that
>
> S-2: S-1 is an empirical proposition about social life . . . Thus do the epistemological claims of the sociology of knowledge collapse in one inescapable and baffling contradiction.[10]

So, we have a thesis that maintains its own falsity.[11] Returning now to the relationship between epistemological relativism and the historicity of the mind, if the former is correct then the latter simply cannot be valid. Which is why, perhaps, Mannheim declared that the two aspects of his theory—the empirical proposition (that thought—the categories in which experiences are subsumed, collected and ordered—varies in accordance with social context) and the epistemological "are not necessarily connected and one can accept the empirical . . . without drawing the epistemological conclusions."[12]

(3) If such conclusions are drawn, however, then it must follow that *all* empirical claims lie on the same plane of credibility. Accepting relativism as our "leading theory of knowledge" means that we can no longer distinguish between correct and incorrect hypotheses. And if there exists no basis for making such distinctions, there is no need to hold our assertions *accountable*. It is not often noticed that epistemological relativism abandons the principle of accountability of assertion.

Turn a moment to the Vienna Circle and ask, what were its members up to when they sought "the Elimination of Metaphysics Through Logical Analysis of Language"?[13] Were they mounting an "onslaught against morals," hoping to banish all ethical and aesthetic discourse? To the untutored eye this is what they seemed to be doing for under the regulatory rule of factual meaningfulness, they did distinguish between the cognitive and noncognitive, and they did take ethical formulations as purely emotive, thereby marking them as "meaningless." Yet there was never any denial that "emotive appeals are indispensable in the pursuits of practical life, in education, in propaganda (good or bad), in poetry, in literature, in religious edification and moral exhortation." And it is clear, Feigl continues, that "some of the highest refinements of our civilized existence depend upon the emotional overtones of spoken and written language."[14] Or, as Ayer put it, "To say that moral statements are not fact-stating is not to say that they are unimportant."[15]

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[7] Robert Merton, *Social Theory and Social Structure* (Glencoe: The Free Press, 1957), p. 503. The original essay appears in the 1949 edition. Emphasis added.

[8] See Virgil Hinshaw, Jr., "The Objectivity of History," *Philosophy of Science*, 25 (January, 1958), 53, emphasis added. See also Hinshaw, "The Epistemological Relevance of Mannheim's Sociology of Knowledge," *Journal of Philosophy*, 40 (February, 1943), 57–72, and "Epistemological Relativism and the Sociology of Knowledge," *Philosophy of Science*, 15 (January, 1948), 4–10; Gustav Bergmann, "Ideology," *Ethics*, 61 (April, 1951), 205–18; Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace & World, 1961), p. 500.

[9] A. R. Louch, *Explanation and Human Action* (Berkeley and Los Angeles: University of California Press, 1966), p. 205.

[10] Benjamin Walter, "The Sociology of Knowledge and the Problem of Objectivity," in *Sociological Theory: Inquiries and Paradigms*, ed. Llewellyn Gross (New York: Harper & Row, 1967), p. 349.

[11] Karl R. Popper, *The Open Society and its Enemies* (London: Routledge and Sons, 1945), Vol. II, p. 217. And see his statement on the "paradox of the liar," pp. 353–4, footnote 7.

[12] Mannheim, *Ideology and Utopia*, pp. 266–69. Quote on p. 267.

[13] Rudolf Carnap in *Logical Positivism*; ed. A. J. Ayer (New York: Free Press, 1959), pp. 60–80.

Now there is little doubt that there were and are emotive overtones (subjectively persuasive connotations) attached to the term "meaningless" which carried over to the term "metaphysical" and to "ethical." This did occur. More therefore, was involved than the use of logic as a neutral tool of analysis. Although the early positivists allowed that there could be many kinds of meaning and many ways of discovering them, the "pathos" evoked had emotive force, conducing to an attraction on a noncognitive basis.[16] This, clearly, was one of the weaknesses of the movement.

Nevertheless, and it bears constant iteration, the Circle inveighed against metaphysical statements not because they contained ethical components; but rather, "for *pretending* to be cognitive, for *masquerading* as something they were not . . . [the] charge against the metaphysician was that he breaks the rules which any utterance must satisfy if it is to be literally significant."[17]

Consider, for example, the following illustration of a "metaphysical pseudostatement" which Carnap presents. It was written by Heidegger, whom Carnap also takes to be representative of the strongest metaphysical school in Germany:

> What is to be investigated is being only and—*nothing* else; being alone, and further—*nothing;* solely being, and beyond being—*nothing. What about this Nothing? . . . Does the Nothing exist only because the Not, i.e., the Negation exists?* Or, is it the other way around? *Does Negation and the Not exist only because the Nothing exists?* . . . We assert: *the Nothing is prior to the Not and the Negation* . . . Where do we seek the Nothing? How do we find the Nothing. . . . We know the Nothing. . . . *Anxiety reveals the Nothing* . . . that for which and because of which we were anxious was 'really'—nothing. Indeed: the Nothing itself—as such—was present. . . . *What about this Nothing? The Nothing itself Nothings.*[18]

We assert, says Heidegger. But what is the assertion, other than "I've got plenty of nothin"? In any case, it is Carnap who proceeds to hold Heidegger *accountable.* By a searching analysis of both the logical syntax of the statement and of its relation to the historical (natural) syntax, he finds that Heidegger's formulation cannot be translated into meaningful sentences of either the natural language or of a logically correct language which can substitute for the latter. The passage stands "as a meaningless sequence of words," as idle chatter.[19]

What I wish to emphasize now is all too often forgotten. Those powerful instruments of criticism which the early positivists employed to call metaphysics to account—they turned to their own work. No privileged status was conferred on any of their own formulations, and no part of their theory was held to be immune from scrutiny. They placed Moore, Russell and Wittgenstein under a microscope—and they did this to each other.

Thus, the verifiability criterion of meaning, for example, was from the first, subject to sustained analysis and criticism. Before long, Schlick's rule of "conclusive verification" (direct and complete) designed to eliminate metaphysics, was shown to have eliminated science—an irony not lost on Reichenbach, Carnap, and Popper. It is now a matter of history that the critical exchanges of the early positivists led to so extensive a relaxation of the original standard that statements, though not testable, were admitted as meaningful if there existed only a possibility of determining the truth of their claims. This failure of the original criteria of meaning, and the weakness of phenomenalism, the inability to expunge metaphysical elements from its own formulations, the radical oversimplification of ethical statements, etc.—all of this is taken to signify the death of positivism.

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[14] Herbert Feigl, "Logical Empiricism," in *Readings in Philosophical Analysis,* ed. Herbert Feigl and Wilfred Sellars (New York: Appleton Century Crofts, 1949), p. 7. This essay was originally published in 1943.

[15] Ayer, p. 22.

[16] For the use of the concept "metaphysical pathos" see Arthur Lovejoy, *The Great Chain of Being* (New York: Harper Torchbook, 1960).

[17] Ayer, pp. 10–11, emphasis added.

[18] Cited by Carnap in Ayer, p. 69. Carnap states that "The thesis that the sentences of metaphysics are meaningless, is to be understood in the sense that they have no cognitive meaning, no assertive content." And he adds, "The obvious psychological fact that they have expressive meaning is thereby not denied" (p. 81). In section 7 of the essay on the "Elimination of Metaphysics," he says: "The (pseudo) statements of metaphysics do not serve for the *description of states of affairs,* neither existing ones (in that case they would be true statements) nor non-existing ones (in that case they would be at least false statements). They serve for the *expression of the general attitude of a person towards life*" (p. 78, emphasis in the original).

[19] John Locke once wrote:

> Vague and insignificant forms of speech, and abuse of language, have so long passed for mysteries of science; and hard or misapplied words with little or no meaning have, by prescription, such a right to be mistaken for deep learning and height of speculation, that it will not be easy to persuade either those who speak or those who hear them, that they are but covers of ignorance and hindrance of true knowledge.

Or as W. S. Gilbert might have said it:

"If you're anxious for to shine in the high *philosophical*
line as a man of learning rare,
You must get up all the germs of the transcendental
terms, and plant them everywhere.
You must lie upon the daisies and discourse in novel
phrases of your complicated state of mind,
The meaning doesn't matter if it's only idle chatter of
a *metaphysical* kind."
Patience (no pun intended), Act. I.

But it is far more appropriate to speak of the Vienna Circle as a stage in the development of the doctrine of accountability—of the demand that all knowledge claims of any sort must overcome the test of a system of external and independent checks. This, in contemporary perspective, is a tradition that is very much alive, and its vitality is due in large measure to the unmitigated honesty of the early positivists who did unto themselves as they did to others—thereby setting us a magnificent example. In the search for an unequivocal rule of objectivity, positivism is to be distinguished from a relativism that, if pursued, prohibits a distinction between sense and non-sense, correct and incorrect, fact and fancy—between truth and error. To make such a distinction is indispensable to science and it is indispensable to society.

(4) To employ the concept of the historicity of the mind to refute the principle of objectivity makes sense only upon the equation of neutrality and objectivity. In political science, as in Miller's paper, the assumption of synonymy permits objectivity to be expressed in terms of a "value-free" observer. Discussion of the problem then turns on "whether or not the social investigator can place his values in abeyance,"[20] i.e., "bracket" them.

There is, however, a profound difference between this formulation and the concept of objectivity. Indeed, it is precisely the enormous difficulty in attaining neutrality that has demanded the system of regulatory control that constitutes "objectivity" in science. That is, science does not require that observers exhibit the pristine purity of total detachment.[21] No one, save perhaps a tyro, seeks to transform a scientist into that fully aseptic and thoroughly neutral observer of legend: this is an impossibility. There is no doubt "that there is more to seeing than meets the eyeball"; that what we see is "theory-laden" or "field-determined." We can admit out of hand that there is no "immaculate perception." Arguments, therefore, which seek to sustain objectivity by predicating neutrality are doomed to fail. And they are quite irrelevant—as is their obverse. Even if neutral observers could be manufactured, Popper tells us, "they could not possibly attain to what we call scientific objectivity."[22] The problem, as Max Born once put it, is to find a way to free oneself from the subjective so as to arrive at objective statements.

That is, the crux of the concept of objectivity rests squarely on the fact that men, even scientific men, are not angels. The entire system of science is based on a variation of Murphy's Law—the prime assumption that any scientist, no matter now careful he may be, is a risky actor; that he is prone to error; that he is not perfectable; that there are no algorithms which he can apply so perfectly as to expunge any and all biasing effects from his formulations. Accordingly, all of his proposals must be subject to error-correcting procedures. By the goals of the enterprise, there is demanded a network of redundant checks[23] to protect against the inclusion of erroneous items in the corpus of knowledge. The process is never ending, and it constitutes an institutionalized control procedure that subjects all knowledge claims to the test of independent and impartial criteria. Needless to say, the relativist denies the possibility of such criteria—resting his position on the historicity of the mind. It remains, then, to show that there are such criteria.

Now we know that veridical truth is a problem that has confronted philosophers (especially philosophers of science) for a long time. Their literature reveals a continuous analysis of such concepts as verification, falsification, justification, warranted assertability, and the like. However these may differ (and it must be added that all too often political scientists do not understand that much of the controversy which attends these concepts turns on the problem of "demarcation"[24]), it is to be emphasized that insofar as contingent (empirical) statements are concerned, whether one is a verificationist and another a falsificationist does not alter the fact that the prime test is the test of experience.

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[20] Fred M. Frohock, *The Nature of Political Inquiry* (Homewood, Illinois: Dorsey Press, 1967), p. 165.

[21] Israel Scheffler, *Science and Subjectivity* (Indianapolis: Bobbs-Merrill, 1967), p. 2. I am much indebted to this brilliant book.

[22] Karl Popper, *Open Society*, p. 217. And see also Popper, *The Logic of Scientific Discovery* (New York: Science Editions, 1961), pp. 44–8.

[23] For an analysis of the theory of redundancy as a theory of error suppression see Martin Landau, "Redundancy, Rationality and the Problem of Duplication and Overlap," *Public Administration Review*, 29 (July, 1969), 346–358.

[24] Popper has written: . . . I shall certainly admit a system as empirical or scientific only if it is capable of being *tested* by experience. These considerations suggest not that the *verifiability* but the *falsifiability* of a system is to be taken as a criterion of demarcation. In other words, I shall not require of a scientific system that it shall be capable of being singled out once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be

There is a necessity to this position which derives from the nature of a hypothesis: For what is a hypothesis but an anticipation of experience? It is a claim against nature, which, by definition, is not known to be true or warranted or confirmed or verified. Accordingly, the only relevant evidence for this claim is precisely the experience which it leads one to expect, which it predicts. The factual status of a hypothesis, thus, can only be determined by examining directly or by consequence, the phenomena it *points* to.

All of this may very well be conceded. But it can be said that the circularity of the observational process, indicated by the use of such terms as "theory-laden," or the Whorf-Sapir principle of linguistic relativity, remains a formidable problem. The problem, of course, is contingent; it is empirical, and it demands evidence. And there is, in fact, a considerable warrant for the suggestion that the act of observation is inherently faulty—that we see only what we are programmed to see. If this is the case, the relation between expectation and observation must perforce be reflexive: observation then becomes a self-fulfilling (or -denying) prophecy, and we can no longer hold an observed experience to be the independent and external control by means of which we render our proposals accountable. The problem may even be more acute. Scheffler has called attention to the "paradox of categorization" which apparently confronts us: if categories of thought (conceptualizations) determine observation, observation cannot then provide an independent control of thought; and if such categories do not determine what is seen, then what is seen cannot provide an independent check because it is uncategorized—formless and nondescript.[25] We do seem to be caught in a double bind—and informed relativists continually call this to attention.

Notice, however, that in this circumstance, no one of us would be able to assess, evaluate, support, or refute any claim. We should have to take all assertions as equally valid and we should have to do away with the concept of error. The whole structure of science, to say the least, collapses by this stroke.

But why restrict the paradox to science alone? Why not apply it to the construct system, the linguistic habits, the "informed" perception of any man in any structured social situation? Does not the bind operate here as well —and by the same dynamic? Yet, if it did, how could there be any change? And how could our linguistic habit contain such categories as error, problem, anomaly, or surprise—not to speak of discovery? If what we see depends only on the "habits of our eyes," then why do "discrepancies" between anticipation and event amaze, astonish, or disturb us, or why—to use a novel locution, do we need any "feedback"? And why do we have such words as "likely" or "probably"?

Is there really any doubt that each of us continually *tests* against experience—that we probe, feel our way, try out—all the time *learning* from our *mistakes*, learning that our conceptualizations are not all that intractable. Notice, too, that when we observe people with conceptualizations that are forever fixed, we say of them, variously, that they are blind, that they have tunnel vision or trained incapacities, that they are prejudiced, and that their categories are "really" stereotypes. Most of us, however, do seem to rely on external empirical checks, for we do discard, amend and revise, and *correct* ourselves. Not always, not easily, but fairly often. And it sometimes happens that we even invent new categories—"feedback" for example. The constant expansion of our vocabulary to accommodate new "things" should certainly give pause.[26]

It is possible, of course, that we are "cultural dopes"—that we are too dumb to understand that we are caught in a paradox; imprisoned by our linguistic categories or the historicity of our minds. No matter.

For we do learn, and the television works, and there are now lasers, space ships, and artificial hormones. So there must be some escape

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> singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience.*

(Popper, *Logic of Scientific Discovery* [pp. 40–41, emphasis in the original]).

[25] Scheffler, *Science and Subjectivity*, p. 13.

[26] It is rather extraordinary that discussions of "objective reality" almost invariably ignore the artifacts of technology and the epistemological implications thereof. Somehow the movement from theory to model to prototype to product does not figure in our discussion—even in the philosophy of science. What happens to the concept of the relativity of knowledge, to the denial of symmetry between thought and thing when Gell-Mann predicts, in all of its characteristics, the omega-minus particle which is then found by means of the Big Machine at Brookhaven? And how in relativism's name did we ever get a proton synchrotron, an airplane, and the paper I write on? It is, alas, a rather unequivocal reality that the atomic bomb destroys.

from the bind. Or, it may be that the equation of our conceptualizations and our observational processes does not hold: that observation does in fact play its regulative role so that we are able to reject an assertion, solve a problem, and design a model city.

To say, then, that conceptualization affects observation is not to say that it necessarily controls observation. But, and this is the point that is lost by those who persist in treating the problem of objectivity in terms of neutrality or value-free observers—it is to call attention to the *fallibility of observers and their potential for error.* As regards the paradox itself, it is easily dissolved upon careful analysis. The trouble is that most of the writing in political science on problems of this kind is pitched in such lofty phrases—and Miller's essay is no exception—as to obscure the problem under consideration. It is not too harsh a judgment to suggest that bibliographic erudition all too often is mistaken for analysis—or that rhetorical grace frequently substitutes for argument. On the latter score, let us build on Scheffler to demonstrate how the apparent contradiction disappears.[27]

In discussions of relativism, the phrase "categories of the mind" looms large; as when it is suggested that such categories "have varied with the succession of epochs and cultures." Then by a chain of reasoning, which Miller describes (items 2 and 3), we are led to the impossibility of objectivity.

But in such discussions, the distinction between a *category* and an *assignment* to the category is invariably ignored—allowing one to conclude that the difference is not all that evident. It becomes evident when we substitute for category such terms as concept, predicate, property: and for assignment, the terms hypothesis, assertion, claim. This distinction has to do with connotation and denotation, or with the closely related (sometimes used synonymously) intension and extension.

Intension refers to the common properties (connotation) that an entity must have in order for a term to be correctly applied to it, to cover it. The extension is the term's range of denotation, of application—the population of things which may be so classified. It should be apparent thus that the extension of a class term is informed by its intension. Admission to the set comes about when criteria of membership are met—which the intension of a category provides. (I take a moment here to suggest that one of the most severe problems in political science, and in political research particularly, is the tendency to make reference to an *extensional* domain through the use of soft and noisy intensional terms.) But it does no more than that. A category may have an intended range of application; it may even presuppose a universe of objects, but by itself it cannot tell us that such objects exist. All that it raises is a "zone of possible occurrences." If a category commits us to anything, therefore, it is to the identification or recognition of instances.

The assignment of an object to a class is another matter. Here we entertain an assertion, a hypothesis. If one says that object A is a legislature and this (class) term has been provided with clear criteria of membership, then one expects to find (predicts) that A exhibits these properties. If it does not, and we can only find this by observation, it cannot be assigned to this class. Assignment thus is only expressible as a hypothesis.

Accordingly, it is the difference between "category" and "assignment" that permits observation to play a regulatory role. The hypothesis is clearly category-laden. It has to be. It is thoroughly informed by categorization. It has to be. And yet it remains subject to an independent check. And while I do not intend any extensive discussion of Feyerabend, it is sometimes supposed that his rejection of the double language model of explanation with its assumption of a "meaning invariant observational core" underwrites a relativism which denies the possibility of such a check. Whether or not this is an inconsistency in a position which advances a theoretical pluralism may very well be questioned. Nevertheless he acknowledges, in a discussion with Hempel, that

> we should have means of correcting the principles of any observation language. From the fact that I interpret concepts in a relativistic fashion, it does not follow that all the predictions I make with the help of the theory that has these concepts will be as the theory says they are. It does not follow that if I interpret the observation language so that it fits the postulates of the theory, then I will not find any observational result incompatible with the theory.

And if there is any doubt here, a little redundancy will help. In a response to Roger Buck, Feyerabend says:

> What you seemed to assume before was that once I interpret the observation language in terms of a theory, observation will never give me a statement inconsistent with the prediction made from the theory. This is not at all the case.

And again:

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[27] *Science and Subjectivity*, chapters 2, 3.

> . . . one who works in an observation language formulated in terms of recent scientific theory is better off because he knows under what circumstances he will be forced to doubt his conceptual system.[28]

And finally, one of the reasons why Feyerabend rejects the double language model is that he thinks that the common-sense conceptual scheme of our ordinary language (the observation core) is wrong: that ordinary language constitutes a theory that is false.[29] Nor need we add that Kuhn's rejection of the concept of "development by linear accretion" rests upon the *rule of observation*. In his view, it is a false hypothesis to assign the history of science to this category. And it is immediately relevant to suggest that his prime categories—normal and revolutionary science—are, in turn, rejected by Popper, Feyerabend, Toulmin, Lakatos and Shapere on the same ground: there is no observational support for Kuhn's assignment.[30]

Elsewhere[31] I have provided an illustration of the way experience alters even our most durable category systems—as, for instance, separation of powers. Even the most elementary text makes it abundantly clear that our observations, clearly category-laden, do not allow for any clear-cut assignments. The units we study under this rubric do not behave "characteristically": They seem to offer a resolute resistance to this time-honored category system. And in the effort to retain it, we have had to introduce so many additional properties as to court "death by qualification." It is for this reason that we have seen so many attempts at theoretical reconstruction (Easton, Deutsch, Almond, Simon, etc.). One usually begins to reconceptualize when one confronts a continual violation of expectation.

It should be clear, then, that the category-system itself can be held accountable, for it is both alterable and replaceable on the basis of observational results. No matter how closely one may adhere to the system (and the closer the better), the assignment of an item to a category always constitutes a hypothesis. And there is no *a priori* way of reducing the uncertainty which attends this claim—i.e., there is nothing which so predetermines the hypothesis as to place it beyond the possibility of error. Nor is there anything which predetermines the distribution of items which fall into the category; and there is nothing to prevent the raising of alternative and even conflicting hypotheses. The same category system can be used to generate sets of competing hypotheses (as in "separation of powers" and Max Weber's system) which, though category-laden, can only be resolved by resort to experience. So it is that observation may be category-laden and still serve as an independent check on hypotheses that are also category-laden. It is certainly the case that there can be no description without prior categories, but these do not so control our descriptions as to place them beyond accountability. *It is accountability, not neutrality, that marks the principle of objectivity.* The "Myth of the Framework" is exactly that—a myth.

Before concluding this discussion, I want to add one more note. Much of the controversy that besets the philosophy of science is often employed by political scientists to warrant one position or another with respect to such problems as explanation. What is obscured rather frequently is the fact that a good deal of this ferment has to do with the effort to find more effective systems of error control. The assumption that seems to prevail is that the construction of a fool-proof or fail-safe system is beyond our reach—that however much we approach a "perfect knowledge," we cannot achieve it. There is, to be sure, a difference between objectivity in principle and in practice.

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[28] See Paul Feyerabend, "Discussion," in *Analyses of Theories and Methods of Physics and Psychology*, ed. Michael Radner and Stephen Winokur, Minnesota Studies in the Philosophy of Science, Vol. 4 (Minneapolis: University of Minnesota Press, 1970), pp. 237–41. See also Feyerabend's "Against Method."

[29] See Paul Feyerabend, "Explanation, Reduction and Empiricism," in *Scientific Explanation, Space and Time*, ed. Herbert Feigl and Grover Maxwell, Minnesota Studies in the Philosophy of Science, Vol. 3 (Minneapolis: University of Minnesota Press, 1962); Feyerabend, "The Problems of Empiricism" in *Beyond the Edge of Certainty*, Robert Colodny, ed. (Englewood Cliffs: Prentice-Hall, 1965); Feyerabend, "Reply to Criticism" in *Boston Studies in the Philosophy of Science*, ed. Robert S. Cohen and Marx W. Wartofsky, Vol. 2 (New York: Humanities Press, 1965), pp. 223–262.

Benjamin Lee Whorf does not think that ordinary language is a *necessarily* false theory but he has observed many times that the user of a natural language generally assumes that it reflects reality more accurately than in fact it does. "Western culture has made, through language, a provisional analysis of reality and, *without correctives*, holds resolutely to that analysis as final." See Whorf, *Collected Papers on Metalinguistics* (Washington, D.C.: Foreign Service Institute, 1952) and J. B. Carroll, ed., *Language, Thought and Reality: Selected Writings of Benjamin Lee Whorf* (New York: John Wiley, 1956), p. 244.

[30] With the exception of Shapere, all of these statements can be found in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970). Dudley Shapere's statement is in "The Structure of Scientific Revolutions," *Philosophical Review*, 73 (1964), 383–394. And see also Stephen Toulmin, "Conceptual Revolutions in Science" in Cohen and Wartofsky, Vol. 3, pp. 331–347.

[31] Landau, *Political Science and Political Theory*.

Nevertheless, the effort to reduce this discrepancy must be made. For this reason, therefore, the concept of independent external check, or experience as arbiter, or "testability" has been expanded so as to comprehend a system of mutual rational control by critical discussion. In fact, Popper has defined science as the "systematic criticism of error."[32] The implication of this is clear: in the absence of such criticism, there is only change; when it constitutes an institutionalized procedure, there is progress. And this extends even to the principle of objectivity. More immediately, what we are to understand is that every theoretical choice, like every hypothesis and theory, is a risky guess. In this regard, Feyerabend insists that even the most well confirmed theory be continually challenged for fear that it will degenerate to dogma. (And, I might add, dogma consists of category-systems which are so constructed as to prohibit the external, independent control of assignment.) Hence he calls for a theoretical proliferation, "a theoretical pluralism [as] an essential feature of all knowledge that claims to be objective." Alternative theories, strong alternatives, are to be so framed that even problems considered to be solved can be subject to re-examination. These provide the *necessary criticism*, a criticism that may even be sharper than the comparison of a theory with a domain of experience. For however much a theory reflects experience, its factual adequacy should come only after a confrontation with strong alternatives.[33] And the reason for this is directly stated: the evidence that is relevant for the test of a theory T can often be unearthed only with the help of an incompatible theory T′. This effort to expand the empirical content of theories has a special purpose, and whether one agrees with Feyerabend or not (his treatment of theoretical pluralism and his use of the pragmatic theory of meaning often give rise to the allegation of relativism) the purpose seems patently clear: "The model which underlies my own discussion has as its aim the maximum testability of our knowledge."[34] I should add that what we have here is a proposal for a multiplexed system of control or the application of the principle of redundancy as a correction to dogma. Whatever its merits, which I find considerable, it does not gainsay the rule of objectivity which requires a strict and continuous regulation on the foundation of systematic criticism and observational results.

(5) My final comment will be brief. It has to do with what Miller calls "the basic document for understanding the new interpretation of the history of science" namely, *The Structure of Scientific Revolutions*.

Kuhn's scenario has overwhelmed political science. Its appeal has been so immediate, so powerful, so universally compelling that it seems as if a massive "conversion experience" has taken place. In the face of the discord and upheaval that characterizes the discipline today, all sides of the "science" controversy derive sustenance and solace from the Book of Kuhn. It is not stretching the metaphor to suggest that it has appeared as a "godsend."

Kuhn's achievement is as monumental as it is perplexing. For what can there be in his theory that permits such divergent, even mutually exclusive positions to rest so heavily on it? The answer to this question requires an analysis of a model of scientific development that has provoked considerable controversy among philosophers of science. Space, of course, does not permit sustained analysis here but I have tried to provide it in another place.[35] Now I wish to enter the caution that before political scientists employ Kuhn's formulations as a court of last resort, it would be well for them to attend to the problems that have been raised:[36]

(a) The descriptive accuracy of Kuhn's history has been challenged, denied and refuted —and by philosophers who are not easily assigned to the classical school (Popper, Feyerabend, Shapere, Toulmin, Lakatos).

(b) The concept of the "paradigm" has been criticized for its terribly ambiguous character (Shapere, Toulmin, Masterman). The elasticity of the term permits anything to be covered, and one sympathetic critic (Masterman) has counted no less than 21 different senses in which it is used. Even Kuhn has admitted that his treatment of "paradigm" in *The Structure of Scientific Revolutions is* "badly confused."[37]

(c) The "conversion thesis" has been denied, attacked, and rejected. (Watkins, Lakatos, Toulmin, Popper.)

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[32] Karl R. Popper, *Conjectures and Refutations* (New York: Harper Torchbook, 1968); see chapter 10.

[33] Paul K. Feyerabend, "How to Be a Good Empiricist," in *Philosophy of Science*, ed. Bernard Baumrin, The Delaware Seminar, Vol. II (New York: John Wiley and Sons, 1963). See also Feyerabend, "Problems of Empiricism"; "Explanation, Reduction and Empiricism," and "Against Method."

[34] Boston Studies, Vol. 2, p. 223.

[35] For an extended analysis of Kuhn, see *Political Theory and Political Science*, chapter 2.

[36] See Lakatos and Musgrave. The entire volume is devoted to Kuhn's book and contains Kuhn's response. And see the essay by Toulmin and discussion by Mink in Cohen and Wartofsky, Vol. 3, pp. 331–355.

[37] Lakatos and Musgrave, p. 234.

(d) The elements of historical relativism present in the analysis—the "Myth of the Framework"—have been shown to hold only in a Pickwickian (simple-minded) sense. Popper has referred to this as a dogma and Scheffler and Lakatos, among others, have shown how easily one may break out. Even the sympathetic Masterman cannot agree here. And Kuhn himself is somewhat equivocal in the face of criticism.[38]

(e) There is so patent a confusion between the normative character of the philosophy of science and the sociological description of scientific practice as to have engendered a new puzzle about Kuhn: does he or doesn't he like normal science? One thing *is* evident though: Kuhn now makes it very clear that there are "good reasons" for the choice of a theory—those stressed by the philosophy of science—"accuracy, scope, simplicity, fruitfulness and the like."[39]

(f) Finally, numerous critics have pointed to the political, psychological and religious metaphors which have structured the book. The immediate implication is that Kuhn's work has been so widely and so well received in the social sciences simply because its *mode* of address is so familiar to us. But naming a phenomenon as revolutionary, as Toulmin would say, neither establishes the fact nor explains it. Here again is the problem of category and assignment and Toulmin, for one, has rejected both.

There are many more criticisms of Kuhn's volume that I need not detail here. But the sum total of these makes it quite clear that while the rhetorical style of the book is very familiar to social scientists, its assertions are not. These demand the consideration of a type of evidence that is beyond our ken—that few political scientists can address with confidence. Accordingly, the exercise of caution in applying Kuhn's formulations to the social sciences is more than warranted.

Would it surprise us to know that to this caveat, Kuhn himself has said Amen!

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[38] Lakatos and Musgrave, p. 264.

[39] Lakatos and Musgrave, p. 261.

Rejoinder to "Comments" by David Braybrooke and Alexander Rosenberg, Richard S. Rudner and Martin Landau

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In "Positivism, Historicism, and Political Inquiry," I argue that the "postbehavioral revolution," as David Easton has termed it, represents something deeper and more durable than a desire for greater attention to practical issues or a commitment to radical politics. This "revolution" gains depth and significance from its repudiation of the positivistic theory of knowledge on which behavioralism rests and its attachment to epistemological principles that have gained ascendancy over positivism in contemporary thought. Easton sees that "postbehavioralism" is quite different in nature from the currents of opposition to behavioralism that had developed in the decade or so before 1965, which he calls "classicism" and "traditionalism." He fails to recognize, however, that it rests on distinctive epistemological presuppositions, which place it in fundamental opposition not only to behavioralism's earlier opponents but also to behavioralism itself.[1] Postbehavioralism has succeeded in identifying some important defects of behavioral approaches to political inquiry, but its underlying epistemological principles are unfavorable to the quest for political knowledge. These principles, if pushed to their conclusion, result in a thoroughgoing relativism, that is, a relativism with respect to "facts" and their relationships as well as to "values."

These observations about current debate in political science are developed in considerable detail in the third section of my essay. In order to support them, it was necessary for me to begin by describing the main features of the view of knowledge which I take to be ascendant in the twentieth century—a view that I, along with many others, have called "historicism." After considering modern epistemology generally, I turn in the second section of the essay to the special field known as the philosophy of science. I argue that the recent revolt in this field against positivistic interpretations of science rests on epistemological premises that are consonant with the account of human knowledge that predominates in modern thought. Attention to developments in the philosophy of science was necessary to illuminate the immediate sources of some antibehavioral arguments in political science.

My present essay incorporates a large amount of material that I discovered after the initial version was written in the summer of 1970. Some of it was generously supplied by writers who knew that I would discuss it critically but who, in a genuinely philosophical spirit, have welcomed a dialogue about the nature of political science. I have searched for material that might disprove my thesis, but nothing that I have observed or read, including the objections raised in this symposium, has led me to conclude that its fundamental argument is wrong. I am aware that still other objections are likely to be raised by those whose views of science or philosophy are different from the view shared by my fellow symposiasts; and I expect that my own research in the epistemological foundations of political inquiry will suggest the need for refinements in my broad thesis. Some may judge that I have ranged more widely than is prudent, but contemporary debate about political inquiry forces those who would understand it to leave their secure havens of specialized research and to explore the most basic questions about knowing and being.

All four of my fellow symposiasts appear to agree with what one of them refers to as "the standard position" in the philosophy of science. This position is an outgrowth of logical positivism or logical empiricism. Given their common epistemological outlook, it is not surprising that their general reactions to my thesis and even their specific criticisms are quite similar. They refuse to concede that positivism is in decline. They dismiss out of hand the suggestion that historicism is the ascendant epistemological position today and that it poses a significant challenge to the prevailing views in the philosophy of science and in political science. If they had taken my case more seriously, they might have interpreted it more accurately and offered stronger reasons for rejecting it. I cannot reply here to each of the criticisms that they do offer, but I shall indicate why I have concluded, after examining these criticisms carefully, that they do not invalidate my thesis.

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[1] "The New Revolution in Political Science," *American Political Science Review*, 63 (December, 1969), 1051–1061. In adopting Easton's term "postbehavioralism," I follow him in emphasizing the fundamental difference between the movement in question and those currents of opposition to behavioralism that favor the revival of political philosophy in its classical mode. Some writers are now using the term "postbehavioralism" to refer to these latter currents. This usage risks blurring the distinction between historicist and nonhistoricist opposition to behavioralism and obscuring the fact that epistemological debate in contemporary political science has three major sides, not two.

Response to Braybrooke and Rosenberg

Professors Braybrooke and Rosenberg direct their arguments not against my actual thesis but against a patently distorted version of it. They interpret my essay to say that positivism has "now been entirely defeated" in twentieth-century epistemology *and* philosophy of science, while historicism has triumphed in *both* areas.[2] Introducing the analogy of warfare to characterize my argument, they would have me say that historicism has won a total victory over positivism and, indeed, has obliterated every trace of positivist opposition.[3]

If I had advanced a sweeping generalization such as this, it could be refuted, of course, merely by showing that positivist interpretations of science continue to be influential in some places or that historicism has not gained a complete victory everywhere. Accordingly, Braybrooke and Rosenberg imagine that they can falsify my thesis by referring to Anglo-American epistemology and philosophy of science, where, they argue, the logical positivists and such figures as Popper and Wittgenstein continue to dominate the scene, along with their students and protégés. They deny that there has been a struggle here between positivism and historicism, claiming that the logical positivists have had no occasion to refer to historicist writers since their original program was formulated. The modifications in this original program are said to have been produced by self-criticism internal to the continuing positivist movement and not by criticism from historicists. Historicism, far from emerging triumphant in English-speaking philosophy, is said to be as nearly totally ignored today as any philosophical movement could be.

In order to judge the effectiveness of this rebuttal, it is necessary to keep in mind the thesis that I actually develop. Let us notice that my essay is careful to distinguish what it says about epistemological debate in modern philosophy generally from what it says about philosophy of science as a special field. Separate sections of the essay are devoted to each area. It is only with regard to the first of these areas—epistemology in the broadest sense—that I say that historicism has become the dominant or ascendant position. This judgment takes account of continental European as well as Anglo-American philosophy. It considers the force of various positions as well as their numbers of avowed supporters. It by no means claims that historicism has become the exclusive epistemological position or that in every country it would receive explicit support from a majority of those who write about philosophy. As for the philosophy of science, I do not say that historicism has been victorious in this field at all.

What I say about modern epistemology obviously cannot be disproved by an argument that focuses on recent philosophy in a limited area and ignores the broad development of Western thought, including European thought, over the past century. Yet this is just the kind of argument that Braybrooke and Rosenberg present. They speak only of epistemological developments in the English-speaking world; and even here, their attention is confined largely to epistemological debate within the philosophy of science or on its periphery. European thought is scarcely mentioned in their essay. They fail to consider the effect on modern philosophy of the radicalization of Hegel's thought or the immense influence of philosophers such as Nietzsche and Heidegger. Their own essay stands as tangible evidence for their observation that these aspects of continental thought are almost totally ignored by English-speaking philosophers.

Braybrooke's and Rosenberg's comments about Anglo-American philosophy, even if entirely sound, would be insufficient to disprove what I say about modern epistemology, but the fact is that these comments are open to serious question. First, our authors give little weight to the massive testimony that logical positivism, though still influential in reconstructions of the logic of science, has ceased to be a dynamic movement in English-speaking philosophy. John Passmore, a respected historian of philosophy, has made the following observations, which I quote at length in the hope of clarifying one of the major points at issue between me and my fellow symposiasts:

> Logical positivism, considered as the doctrine of a sect, has disintegrated. In various ways it has been absorbed into the international movement of

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[2] David Braybrooke and Alexander Rosenberg, "Getting the War News Straight: The Actual Situation in the Philosophy of Science," *American Political Science Review*, 66 (September, 1972), p. 818.

[3] They suggest, for example, that my essay is the source of rumors that positivists have "vanished" from the philosophy of science, although at other times I am made only to say that the fall of positivism is "impending."

contemporary empiricism, within which the disputes which divided it are still being fought out. Originally, it set up a series of sharp contrasts: between metaphysics and science, logical and factual truths, the verifiable and the nonverifiable, the corrigible and the incorrigible, what can be shown and what can be said, facts and theories. In recent philosophy, all these contrasts have come under attack, not from metaphysicians but from philosophers who would in a general sense be happy enough to describe themselves as "logical empiricists." Even among those philosophers who would still wish to make the contrasts on which the logical positivists insisted, few would believe that they can be made with the sharpness or the ease with which the logical positivists at first suggested.

Logical positivism, then, is dead, or as dead as a philosophical movement ever becomes. But it has left a legacy behind. In the German-speaking countries, indeed, it wholly failed; German philosophy, as exhibited in the works of Heidegger and his disciples, represents everything to which the positivists were most bitterly opposed. In the United States, Great Britain, Australia, the Scandinavian countries, and in other countries where empiricism is widespread, it is often hard to distinguish the direct influence of the positivists from the influence of such allied philosophers as Russell, the Polish logicians, and the British "analysts." But insofar as it is widely agreed that transcendental metaphysics, if not meaningless, is at least otiose, that philosophers ought to set an example of precision and clarity, that philosophy should make use of technical devices, deriving from logic, in order to solve problems relating to the philosophy of science, that philosophy is not about "the world" but about the language through which men speak about the world, we can detect in contemporary philosophy, at least, the persistence of the spirit which inspired the Vienna circle.[4]

As Passmore's statement shows, one may plausibly speak of the demise of logical positivism as a viable movement in philosophy even while recognizing that a great many empiricists, inspired by its spirit, continue to debate the issues that it raised. Braybrooke and Rosenberg prove, at most, that logical positivism has left a legacy behind. They certainly do not prove that logical positivism is a vital and coherent force in contemporary philosophy. Their argument here is somewhat inconsistent, for they wish to deny that logical positivism is in decline even while admitting that crucial parts of its original program are no longer regarded as tenable. Throughout their essay, we are led to believe that the "continuing positivist movement" is alive and flourishing. At the end, however, we are told that

> [i]n the present context, to meet Miller on his own ground and simplify our exposition, we have freely used the terms "positivism" and "positivists"; but philosophers—at any rate, philosophers of science in the English-speaking world—do not think of themselves as positivists or even as postpositivists. They sometimes use the term "positivism" to designate doctrines left behind, several transformations ago, in the stream of mixed doctrines now current as evolutionary descendants of "positivism."[5]

Does this concluding admission not bring their entire analysis into question and indicate that I was correct in speaking of the decline of positivism?

Braybrooke and Rosenberg fail to consider the possibility that the changes which they record in the positivist movement have led not merely to "transformations" in the movement but to its deterioration. They are blind also to the implications of these changes for positivism's struggle against historicism. As I shall show later, the Vienna Circle's case against historicism rested on its claim that a clear line of demarcation could be drawn between empirical science, whose statements are meaningful, and the domain of metaphysics and value theory, whose statements are cognitively meaningless. Our authors point out that C. G. Hempel, whose work they take to be representative of developments in the positivist program, decided in the 1950s that cognitively meaningful and meaningless sentences could not be distinguished by reference to a principle of verification. Hempel adopted new criteria for determining when a term or sentence has cognitive significance, drawing in part from Carnap's work on confirmation; but as Braybrooke and Rosenberg observe, there is now a large body of literature which holds that Carnap's theory of confirmation is unsuccessful. Hempel has also surrendered other central positivist doctrines that were once deemed essential to the distinction between scientific and nonscientific modes of thought. It does not occur to Braybrooke and Rosenberg that the abandonment of these central doctrines might have produced a crisis for positivism and impaired its ability to resist historicist views of knowledge. Let me add that insofar as the validity of my thesis is concerned, it is irrelevant whether these modifications in the positivist movement came about from internal self-criticism or were motivated by criticism from historicists.

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[4] "Logical Positivism" in *Encyclopedia of Philosophy* (New York: Macmillan and The Free Press, 1967), V, 56. In the Preface to *The Legacy of Logical Positivism* (Baltimore: Johns Hopkins Press, 1969), the editors, Peter Achinstein and Stephen F. Barker, write: "Today logical positivism no longer exists as a distinct movement, yet its effects, direct and indirect, recognized and unrecognized, continue to be felt" (p. v). As for European philosophy, Ludwig Landgrebe states: "If we want, first of all, to describe the common ground on which European philosophy and the thinkers determined by it rest, we may say that it is characterized throughout by the elimination of an attitude which had gained its maximum momentum at the end of the nineteenth century, an attitude which can be designated as *positivistic* in the most comprehensive sense of the term." In *Major Problems in Contemporary European Philosophy*, trans. by Kurt F. Reinhardt (New York: Ungar, 1966), p. 6, italics in the original. If necessary, one could produce a great number of statements by competent observers attesting to positivism's decline as a philosophical force.

[5] Braybrooke and Rosenberg, "Getting the War News Straight," p. 825.

A second deficiency in Braybrooke's and Rosenberg's account of Anglo-American philosophy is their failure to recognize the extent to which it has been influenced by principles at least implicitly historicist in nature. Our authors identify two primary traditions in Anglo-American philosophy: the positivist tradition and "the other analytic tradition," that is, ordinary language analysis as developed by the followers of the later Wittgenstein. Can we simply take it for granted, as Braybrooke and Rosenberg do, that ordinary language philosophy is nonhistoricist? Stanley Rosen has argued—quite persuasively, I believe—that ordinary language philosophy is recognizable as a version of historicism when its assumptions and implications are spelled out fully.[6] We have no evidence that Braybrooke and Rosenberg have given thought to this possibility. They certainly give no reasons for rejecting it.

My essay points to some historicist implications of Wittgenstein's later thought. In their cursory reply to my interpretation of Wittgenstein,[7] Braybrooke and Rosenberg begin by asserting that it would be a mistake to suppose that Wittgenstein was influenced at all by historicist philosophers. Stephen Toulmin has recently shown, however, that Wittgenstein's thought is rooted in the German tradition that gave rise to historicism and that it resembles "post-Kantian Existentialism" in some important respects. Toulmin points out that *Lebensformen* (Forms of Life) was the title of a widely circulated book by Eduard Spranger which was first published in 1914. We know from other sources that Spranger was a leading spokesman for Dilthey's historicist school.[8] Furthermore, several writers have called attention to the areas of agreement between Wittgenstein's position on language and that of Dilthey and Heidegger.[9] Braybrooke and Rosenberg assert that an interpretation of Wittgenstein such as the one I offer has not become standard. This is no proof, of course, that the interpretation is wrong. Finally, they observe that many language games are copresent "in our culture," so that they are not simply identical to the *Weltanschauungen* of historicist writers. Yet even assuming that each of us is covered by a multiplicity of life-forms, the pattern or "shape" of the totality is still a conventional construction, which shifts with the historical winds. The partial agreement of language games within a culture in no way rules out a relativity of linguistic habits or worldviews *among* cultures.

Braybrooke and Rosenberg take my statements about the ascendancy of historicism in modern epistemology and make them apply also to what I say about the philosophy of science. Indeed, they suggest at one point that my essay is the source of "rumors that the positivists have *vanished* from the philosophy of science and left the field to historicists."[10] It is rumors such as these that their comments are designed to refute. They concede that there has been an "uprising" in the philosophy of science but hold that the rebels are very far from winning their main points. They maintain that interpretations of science such as those of Kuhn and Feyerabend are rejected by a majority of philosophers of science and are, in any event, neither historicist in character nor comprehensively antipositivist.

Even a cursory reading of my essay should disclose that this interpretation of what I say about the philosophy of science represents a gross distortion. I argue that a revolt has occurred there against positivistic interpretations of science. I speak of a cleavage having developed, of positivism having come under siege finally in its last stronghold, and of an antipositivist movement. These phrases certainly do not imply that the issue has been *settled* or that the antipositivism of writers such as Kuhn and Feyerabend has become the exclusive or even the predominant position. The strongest term I use, "revolution," appears in a quotation from a widely cited article by a philosopher of science, Dudley Shapere.[11] Braybrooke and Rosenberg declare that Shapere himself attacks the antipositivist position, thinking that they have shown thereby that my own sources contradict my alleged thesis that the antipositivists have overcome all opposition. But my essay states clearly that Shapere is critical of the antipositivist views which he describes. Furthermore, I cite other critics of these views—critics such as Scheffler, Achinstein and Kordig. My essay attributes no greater influence to the views of such antipositivists as Kuhn and Feyerabend than does the essay of Shapere, which my critics generally applaud.

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[6] See Stanley Rosen, *Nihilism* (New Haven: Yale University Press, 1969), pp. 1–27.

[7] Braybrooke and Rosenberg, "Getting the War News Straight," pp. 820–821.

[8] See Stephen Toulmin, "Ludwig Wittgenstein," *Encounter*, 32 (January, 1969), 58–71. Spranger's association with the school of Dilthey is noted by Werner Brock, *Contemporary German Philosophy* (Cambridge: University Press, 1935), p. 92.

[9] See Karl-Otto Apel, *Analytic Philosophy and the Geisteswissenschaften*, trans. by Harald Holstelilie (Dordrecht, Holland: Reidel, 1967) and Arne Naess, *Four Modern Philosophers: Carnap, Wittgenstein, Heidegger, Sartre*, trans. by Alastair Hannay (Chicago: University of Chicago Press, 1968), pp. 167–171.

[10] Braybrooke and Rosenberg, "Getting the War News Straight," p. 824. Emphasis added.

In summarizing the antipositivist position, Shapere calls attention to its view that scientific inquiry is fundamentally dependent on presuppositions that are historically variable and unamenable to either rational proof or empirical verification. He speaks also of its relativistic implications. I point out the similarity of this view to classic statements of the historicist position, but clearly I do not attribute this view to all philosophers of science, nor hold that it is identical in every detail to broader historicist statements, nor insist that it repudiates every aspect of the positivist program. My critics' arguments against such assertions as these have no application to what I actually say. Furthermore, I do not endorse the epistemological relativism that one finds in the writings of Kuhn and Feyerabend, as Braybrooke and Rosenberg perhaps assume. I share Braybrooke's and Rosenberg's desire to find sound arguments against epistemological relativism, but I doubt that a satisfactory refutation can be established upon a positivistic account of reason and experience.

Braybrooke and Rosenberg are astonished by my argument that the new antipositivism in the philosophy of science gives expression to much broader historicist tendencies in modern epistemology. They declare that none of the antipositivist claims is taken by their defenders *or their objectors* to represent an historicist reinterpretation of modern science. Yet Popper, in one of the very books they cite, states flatly that "Kuhn's logic is the logic of *historical relativism*."[12] If my critics knew as much as Popper does about historicism, it is not likely that they would have felt such astonishment at the suggestion that it resembles and may have influenced the antipositivist movement in the philosophy of science.

Much of what I say later will bear upon Braybrooke's and Rosenberg's critique, especially upon their contention that the evolution of logical positivism has in no sense reflected a struggle against historicism. Before leaving it, however, I wish to comment on their concluding suggestion that political scientists should leave disputes about philosophical doctrines to philosophers and turn their attention instead to the study of actual political phenomena. It is in this way, they suggest, that political scientists can best establish the worth of alternative methods and make a genuine contribution to philosophy. The political scientist's attempt to speak directly to philosophical debate or to choose sides in it is bound to result in "paraphilosophy": "He cannot expect to follow a doctrine through all its transformations; or to identify precisely the key issues that lead from one transformation to another."[13]

I agree that the political scientist's primary business is to understand politics. The worth of any method of political inquiry must be judged by its success in helping us to understand the political phenomena that present themselves to the natural or prephilosophic consciousness. Braybrooke and Rosenberg must know, however, that it is very difficult today for a political scientist to establish a direct relationship to the phenomena of political life and to see political things as they are. From his undergraduate years, the political scientist is taught to see politics in terms of certain methodologies and conceptual frameworks. These become filters or screens that restrict and distort his vision of political reality. Before he can see political things as they are and understand them, the political scientist must identify his inherited opinions about politics and science and subject them to critical scrutiny. These inherited opinions are but residues and abbreviations of comprehensive philosophical statements that originated in the past. In order to clarify his opinions, therefore, the political scientist is forced to give attention to the history of philosophy, where speculation about politics is inseparable from speculation about the character of human knowledge. By working his way back to the sources of his opinions about political things and how to study them, he is able to understand these opinions more clearly and to assess them more accurately. If he reaches the point in time when political philosophy originated, he discovers a mode of thought that enjoyed a direct relationship to political life. The study of political philosophy in its classical mode can facilitate his own effort to see political things as they are.

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[11] It is clear that Shapere, in using the term "revolution," has in mind the radical break that the new antipositivism makes with established views in the philosophy of science and the intense debate that the antipositivist critique has occasioned, not any overwhelming victory that the movement has achieved. I believe that it is equally clear in my essay that I have the same meaning in mind on those few occasions when I follow Shapere in using the term. I adopt Easton's term "postbehavioral revolution" in much the same way.

[12] Karl Popper, "Normal Science and its Dangers," in *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave (Cambridge: University Press, 1970), p. 55, italics in the original.

[13] Braybrooke and Rosenberg, "Getting the War News Straight," p. 825.

The philosophy of science is one major source of the prevailing opinions in political science about methodology. Alternative approaches to political inquiry are not likely to receive serious consideration from political scientists until these opinions are submitted to a radical critique, that is, a critique that goes to their roots or foundations. Braybrooke and Rosenberg are quite unrealistic in proposing that political scientists simply pursue their studies, as natural scientists do, without great concern for what happens in the philosophy of science. This discipline has had little effect on the practice of natural science, but it has exercised a tremendous infuence over the minds of political scientists, who have little choice now but to keep up with developments in the philosophy of science and to choose sides in its disputes.

The investigation of philosophical debate and its history is now virtually a precondition for gaining a fresh and direct understanding of political life. When the political scientist attempts by this means to rethink his opinions about political things and how they should be studied, he is engaging in philosophy. I regard my own essay as philosophical in this sense,[14] but Braybrooke and Rosenberg prefer to call it "paraphilosophy." Unfortunately, they fail to say enough about either the meaning of this term or their understanding of philosophy to give us a clear indication of just what "paraphilosophy" is. The prefix can mean both "that which is beside or contrary to" and "that which shields or protects from." I suggest that it is Braybrooke's and Rosenberg's position, and not my own, which stands against philosophy by diverting it from inquiry into the nature of things to a preoccupation with the logic of a particular mode of inquiry and which shields political scientists from involvement in philosophy. Political scientists already know too much about philosophical debate and, at the same time, too little simply to detach themselves from it. Moreover, I see no evidence that political scientists are less competent in identifying and understanding the fundamental issues of philosophy than, for example, philosophers of science are.

Response to Rudner

Upon reading Professor Rudner's lengthy critique, which was the last of the three to reach me, I was struck immediately by the extent to which it resembles that of Braybrooke and Rosenberg. I see only one significant point where Rudner disagrees with them: he believes that "it is sensible for serious social scientists to be *au courant* in contemporary philosophy of science (and, indeed, other aspects of philosophy)."[15]

The first part of Rudner's critique is designed to show that I have "made a drastically mistaken reading of the current and recent situation in philosophy of science."[16] Alleging, as Braybrooke and Rosenberg do, that "Miller purports to bring word of the ascendancy, over positivism, of a view he calls *historicism* in epistemology *and* philosophy of science,"[17] Rudner sets out to refute "the contention that recent developments in philosophy of science are properly characterizable as the triumph of historicism over positivism."[18] In subsequent passages, he has me say that historicists have won a "victory" in the philosophy of science, that positivism is "moribund" there, that historicism "dominates" the field or has been left "holding" it, and that "the standard position" in philosophy of science "has been shattered and superseded by an historicist position."[19] Rudner's restatement of my view is particularly misleading because it puts such terms as "field-overwhelming," "defeating" and "victory" in quotation marks, thereby falsely implying that they appear in my own references to the philosophy of science.[20]

We see that Rudner builds his case, as Braybrooke and Rosenberg do, on the claim that I misrepresent the situation in the field of philosophy of science by holding that the historicism of writers such as Kuhn and Feyerabend has become the dominant or victorious position there. I believe that a fair-minded reading of my essay as a whole will show that this interpretation is unjustifiable. While I argue that historicist views are ascendant in modern epistemology generally, I make no such claim about the specific field called "the philosophy of science." As I point out in my essay, the methodological literature in political science has treated a positivistic model of scientific inquiry as though it enjoyed the full endorsement of philosophers of science. I wished to show that positivistic accounts of science have come under strong attack within the philosophy of science itself from a vigorous antipositivist movement. It was obvious to me as I prepared my essay that the new antipositivism would not presently be endorsed by anything approaching a majority of philosophers of science. I might have emphasized this point if it had not been so obvious to me and—as I supposed—to my readers.

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[14] Rudner comes close to the truth when he speaks of my essay as a "dialectical analysis." His mistake is to regard it as dialectical in an Hegelian rather than a Platonic sense.

[15] Richard S. Rudner, "On Evolving Standard Views in Philosophy of Science," *American Political Science Review*, 66 (Sept. 1972), p. 827.

[16] Rudner, "On Evolving Standard Views," p. 828.

[17] Rudner, "On Evolving Standard Views," p. 827. The word "and" not italicized in the original.

[18] Rudner, "On Evolving Standard Views," p. 828. Emphasis added.

[19] Rudner, "On Evolving Standard Views," p. 832.

[20] See Rudner, "On Evolving Standard Views," p. 839.

Perhaps my critics' misinterpretation of my essay on this point arises from their narrow understanding of the scope of epistemology. They assume that the only relevant epistemological speculation has occurred within the philosophy of science or on its periphery. It would thus be quite natural for them to conclude, when I speak of the ascendancy of historicism in modern epistemology, that I have in mind philosophy of science as well. My essay indicates, however, that the most important and influential developments have occurred some distance from this field.

Rudner's critique follows the same pattern as that of Braybrooke and Rosenberg. First, he limits his attention to English-speaking philosophy. Yet as we have seen, it is altogether impossible to evaluate the thesis about modern epistemology that I actually develop without giving attention to philosophical currents that have predominated in Europe.

In the second place, Rudner's discussion of English-speaking philosophy, like that of Braybrooke and Rosenberg, centers on the philosophy of science. He argues that the "standard position" in the field is one that has evolved out of the early work of the Vienna Circle under the impetus of a continuing debate within logical empiricism itself. He grants that this standard position has been criticized sharply by "the history-of-science-school," but holds that it is still the established viewpoint. The point to be noted here is that Rudner's assessment does not contradict my essay, for I do not say, as he would have the reader believe, that "the history-of-science-school" has overwhelmed the field and defeated its rivals.

Finally, Rudner, like Braybrooke and Rosenberg, doubts that the views either of Wittgenstein or of Kuhn can properly be considered historicist. Yet I find nothing whatsoever in Rudner's discussion of these writers to support his charge that I commit "crucial mistakes respecting the import or implications of their views."[21] There is no merit to his argument that an alleged disagreement between Wittgenstein and Kuhn on one point—the possibility of "rational reconstruction" of the essential functions of scientific behavior—shows "that they could not both, consistently, be irrationalist-historicists."[22] Whatever their disagreements, they hold that the horizons of meaningful speech and thought are set by linguistic conventions, values, or presuppositions that vary from one historical setting to another; and they deny that men can, by reason or experience, transcend these variable and contradictory horizons to discover truth about nature or meaning in nature—a truth or meaning that would serve as the basis for an assessment of the specific life-forms or paradigms.

Rudner sees nothing in Wittgenstein's teaching that implies an historicist position about science. That teaching leads, however, to the conclusion that science is radically conventional and relative to the historical variety of conventions. Any account which scientists give of something, including a *Lebensform,* must, as an act of speech, be a matter of conventional agreement. The account cannot be verified by pointing to the facts or to the nature of the thing because there is no "fact" or "nature" or "meaning" prior to linguistic agreement within a contingent *Lebensform.*

Rudner points out that Kuhn has restated his position under the pressure of criticism. He quotes at great length from one of Kuhn's restatements in order to show that Kuhn would reject the charge of relativism as I have formulated it. Yet Rudner conveniently skips over those passages that are most pertinent to the present discussion. Kuhn does indeed hold that theories are chosen on the basis of "good reasons," but he concedes that if scientists hold different "values"—and presumably they will from one paradigm to another—they will come to different conclusions about what reasons, and hence what choices, are "good." Furthermore, Kuhn admits that "in one sense of the term I may be a relativist," although he does not believe this to be true in a more essential sense. He goes on to explain that he is not a relativist in regard to the utility of scientific theories, for he is prepared to say that one theory is better than another as a tool for the practice of normal science.

[21] Rudner, "On Evolving Standard Views," p. 841.
[22] Rudner, "On Evolving Standard Views," p. 840.

> Nevertheless, there is another step, or kind of step, which many philosophers of science wish to take and which I refuse. They wish, that is, to compare theories as representations of nature, as statements about "what is really out there." Granting that neither theory of a historical pair is true, they nonetheless seek a sense in which the later is a better approximation to the truth. I believe nothing of that sort can be found.[23]

It is precisely this kind of relativism—the essential kind insofar as epistemology is concerned—that I had in mind when discussing Kuhn in my essay.

Rudner puts great emphasis on what he calls my "errors of omission," especially my failure to give attention to the "standard position" in philosophy of science. He would have the reader believe that my observations about the antipositivist revolt were intended as a full account of all that has been happening in the field. This wholly unwarranted assumption allows him to claim that my omissions are in fact "errors." My essay makes it abundantly clear that its focus is limited to one movement in philosophy of science—the antipositivist movement—which has emerged to oppose the dominant positivism in the field. I turned to the philosophy of science to shed light on the protest against behavioralism in political science; and I felt justified in limiting my attention to the antipositivist position because this is the one that has influenced some of the "postbehavioralists." I assumed that most readers of this Review would be sufficiently familiar with the "standard position" to find my essay intelligible.

Rudner attempts to make up for my "errors of omission" by providing his own account of the "standard position" and its development. This enterprise, if properly executed, could throw valuable light on our discussion. Unfortunately, Rudner's summary, while giving some important details about prevailing views in the philosophy of science in three problem areas, fails to confront the problem that is central to the dispute between positivism and historicism, namely, the problem of whether or not the cognitive claims made by or on behalf of modern science can be upheld on epistemological grounds. One aspect of this problem is whether the logic of modern science and its underlying assumptions about causality and explanation have a foundation in reason or nature or whether one's choice of them is finally arbitrary. Another aspect is whether or not experience has the character that would be required to support the claim that empirical science is the definitive mode of cognition. Rudner is aware that logical positivism's original account of the nature of scientific theories and the logic of their validation rested upon a distinctive view of experience. He tells us that this view of experience was partly abandoned in the "liberalization" of positivism that produced the "standard conception" of science, but he fails to ask if this "empirically weakened" conception of science can defend itself successfully against historicism. He also fails to consider how Hempel's recent proposal for amending the "standard conception" would affect its viability.

I cannot give detailed consideration here to the evolution of the positivist account of science, but I believe that I can indicate, more clearly than Rudner has, just what aspects of that evolution are essential to our discussion. For this purpose, I shall look briefly at the work of Rudolf Carnap, who has been the dominant figure in the logical positivist movement. My discussion of Carnap will provide grounds for deciding whether or not a struggle against historicism took place in the course of logical positivism's evolution. Rudner insists, as Braybrooke and Rosenberg do, that this evolution took place under the impetus of a continuing internal critique by the logical positivists themselves and philosophers sympathetic to them. This allegedly self-contained evolution of logical positivism is cited by my critics as proof that no significant conflict or warfare has occurred between this movement and historicism.

If one reads Carnap's valuable account of his intellectual development[24] with our present problem in mind, he will be impressed by the extent to which Carnap was exposed as a young man to counterempiricist and even historicist currents of European philosophy. Carnap speaks, for example, of an intensive study of Kant, of a seminar on Hegel, and of the influence which the neo-Kantians Natorp and Cassirer had on his dissertation. He remembers with special pleasure and gratitude the seminars of Hermann Nohl. We learn from other sources that Nohl was a student of Wilhelm Dilthey and a proponent of Dilthey's historicist views.[25] This is not to say that Carnap adopted the views of these men. The writings of Mach

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[23] Thomas S. Kuhn, "Reflections on my Critics," in *Criticism and the Growth of Knowledge*, p. 265. See also pp. 261–264. My other critics also cite this recent essay of Kuhn's but they, like Rudner, skip over this crucial statement.

[24] Rudolf Carnap, "Intellectual Autobiography," in *The Philosophy of Rudolf Carnap*, edited by Paul Arthur Schilpp (La Salle, Ill.: Open Court, 1963), pp. 3–84.

[25] See Naess, *Four Modern Philosophers*, p. 43.

Russell, and the early Wittgenstein were to prove far more influential in shaping his theory of knowledge. Yet we can at least say that he was familiar with the alternatives to empiricism; and we shall find that he did not reject them entirely.

Carnap's early position was formulated in express opposition to German historicism. He agreed with the Vienna Circle's proposal for a unified science, which, he tells us, "must be understood primarily as a rejection of the prevailing view in German contemporary philosophy that there is a fundamental difference between the natural sciences and the *Geisteswissenschaften* . . . "[26] Also of great importance is his essay on "The Elimination of Metaphysics Through Logical Analysis of Language,"[27] which grew out of a 1930 lecture. The essay mentions Hegel and Nietzsche, but its major opponent is Heidegger, whose work Carnap cites as the exemplification of "that metaphysical school which at present exerts the strongest influence in Germany."[28]

Two features of Carnap's essay are particularly pertinent to our present discussion. First, he draws a sharp distinction between the meaningful statements of factual science and the meaningless "pseudo-statements" of metaphysics. Crucial to his distinction is the claim that the former statements refer to the domain of experience and are thus verifiable, while the latter pretend to knowledge that reaches above or behind experience. Insofar as the nature of experience is concerned, he observes that "[i]n the theory of knowledge it is customary to say that the primary sentences refer to 'the given,' " though he adds that "there is no unanimity on the question what it is that is given."[29] For further elaboration of his distinction between science and metaphysics, he refers the reader to Wittgenstein's *Tractatus* and to his own *Logischer Aufbau,* where he had argued that statements, as a condition of their meaningfulness, must be verifiable, that is, reducible to elementary propositions which refer to sense perceptions that are known directly and with certainty.

The second feature of interest to us in Carnap's essay is his argument that metaphysical statements serve not for the description of states of affairs but "for the *expression of the general attitude of a person towards life* ('Lebenseinstellung, Lebensgefühl')."[30] This attitude "manifests itself, unconsciously as a rule, in everything a man does or says." Music, art, and metaphysics are to be understood as special expressions of this basic "attitude in life"—a term which Carnap tells us that he prefers to the more ambiguous term "Weltanschauung."[31] As Arne Naess points out, this characterization of metaphysics owes a great deal to Dilthey's thought, which Carnap had come to know through his teacher Hermann Nohl.[32] In fact, Carnap calls attention to the work of Dilthey and his students in this essay.

We see that while the early Carnap opposed historicism on some essential points, he agreed with it on others. He was quite willing to concede that an historicist account of human thought is correct insofar as metaphysics is concerned. Metaphysics is but the expression of the basic "attitude in life" which manifests itself, unconsciously as a rule, in *everything* a man does or says. Although his use of the term "everything" would seem to bring even natural science within the sphere of human things that give expression to their author's attitude in life, Carnap maintains nonetheless that science and metaphysics are fundamentally different in their cognitive status. Scientific statements, unlike those of metaphysics, can be cognitively meaningful because they are grounded in the experientially given. Carnap grants that there are different interpretations of just what is "given," but his entire argument seems to depend on the assumption that the "given" of sensory experience, whatever it is, has a fixed and ascertainable meaning.

As Rudner notes, Carnap would gradually abandon the epistemological position that he had taken in the *Logischer Aufbau.* For our purposes, it is highly significant that Carnap rejected his earlier view according to which the experiential base of science consists of immediate and certain knowledge. He gives the following description of his early theory of knowledge in his intellectual autobiography:

> According to the original conception, the system of knowledge, although growing constantly more comprehensive, was regarded as a closed system in the following sense. We assumed that there was a certain rock bottom of knowledge, the knowledge of the immediately given, which was indubitable. Every other kind of knowledge was supposed to be firmly supported by this basis and therefore like-

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[26] Rudolf Carnap, "Intellectual Autobiography," p. 52.

[27] Reprinted in A. J. Ayer, ed., *Logical Positivism* (Glencoe: The Free Press, 1959), pp. 60–81. The essay first appeared in 1932 in *Erkenntnis* under the title "Überwindung der Metaphysik durch logische Analyse der Sprache."

[28] Carnap, "The Elimination of Metaphysics," p. 69.

[29] Carnap, "The Elimination of Metaphysics," p. 63.

[30] Carnap, "The Elimination of Metaphysics," p. 78. Italics in the original.

[31] Carnap, "The Elimination of Metaphysics," p. 79.

[32] Naess, *Four Modern Philosophers,* pp. 41–47. Naess brings out very clearly the areas of agreement and disagreement between Carnap and Dilthey in this area.

> wise decidable with certainty. This was the picture which I had given in the *Logischer Aufbau;* it was supported by the influence of Mach's doctrine of the sensations as the elements of all knowledge, by Russell's logical atomism, and finally by Wittgenstein's thesis that all propositions are truth-functions of the elementary propositions.[33]

The fundamental question that now arises, however, is this: Does Carnap's abandonment of his early account of experience not place in jeopardy the strict distinction that he had attempted to draw between science and metaphysics and thus invite the argument that science can claim no exemption from the historicist account of human thought—an account which Carnap had been willing to adopt insofar as metaphysics is concerned? My critics, in discussing the changes in Carnap's thought, are completely oblivious to this question.

Let me summarize the problem raised by the "turn" in Carnap's thinking. In the late 1920s and early 1930s, Carnap had tried to establish a clear-cut distinction between science and metaphysics on epistemological grounds. Crucial to his distinction was the argument that science has a "certain rock bottom of knowledge" in the given data of experience. By coming to deny that experience supplies us with immediate and indubitable knowledge, he called into question the epistemological basis of his own distinction between science and metaphysics. (Carnap had already eliminated the possibility of basing this distinction on the cognitive powers of reason by deciding that reason as such cannot inform us about the factual world.) The early Carnap had drawn upon the historicist principles of Dilthey to characterize metaphysical thought and, indeed, "everything a man does or says." By surrendering the view of experience on which his distinction between science and metaphysics was based, Carnap opened the door to the claim that the historicist account of human thought applies to science as well as to metaphysics. He would, of course, continue to hold that scientific knowledge is rooted in experience. He would go on to develop his principle of confirmability as an empiricist criterion of significance. Yet the appeal to the data of experience loses its force if the possibility exists that these data are known to us only as conditioned by historical or cultural influences or if they are not the primary source of meaning. Rudner tells us that contemporary positivists are willing to concede that perceptions are conditioned by factors or circumstances of the social and natural environment, but he does not face the problems that this concession raises as regards the objectivity and reliability of empirical science.[34]

It is clear from Carnap's writings that a struggle against historicism was built into the program of logical positivism from the very beginning. Whether they realize it or not, the figurative children and grandchildren of the Vienna Circle continue to struggle against historicism when they affirm some of the Circle's characteristic doctrines, such as the methodological unity of natural and social science and the strict difference between science and metaphysics. It is true, as my critics say, that those within the positivist tradition have abandoned or modified much of the Circle's original program. My critics have the curious notion that I am obliged to show that these changes were somehow *produced* by debate with historicists. The real issue, however, is whether or not these changes—whatever their source—have weakened positivism's capacity to oppose historicism effectively. I have attempted to speak to this issue in my comments on Carnap. My critics, however, ignore it completely.

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[33] Rudolf Carnap, "Intellectual Autobiography," p. 57.

[34] It would take us too far afield to explore the issue of whether Carnap, in his later writings, succeeds in upholding the distinction between science and metaphysics and thus in maintaining the nonhistoricist character of scientific knowledge. Karl Popper argues that Carnap's "testability" (or "confirmability") criterion of meaning is no more successful than his earlier verifiability criterion in distinguishing science from metaphysics. See Popper's "The Demarcation Between Science and Metaphysics," in *The Philosophy of Rudolf Carnap,* pp. 183–226. See also, in the same volume (pp. 99–158), Robert S. Cohen's "Dialectical Materialism and Carnap's Logical Empiricism," which characterizes Carnap's confirmation criterion as "a form of relativist conventionalism" because it leaves to chance, or simply treats as axiomatic, the intersubjective agreement of scientists about observation statements. Again in this volume (pp. 227–267), Herbert Feigl, in an essay entitled "Physicalism, Unity of Science and the Foundations of Psychology," gives the following interpretation of his friend Carnap's fundamental principle of empiricism: "Statements are to be regarded as *scientifically meaningful* only if they are in principle intersubjectively confirmable or disconfirmable. If a statement, by the very interpretation imposed upon it, is in principle incapable even of the most indirect sort of intersubjective test, then though it may have meaning of a purely logical sort, or may be significant in that it carries pictorial, emotional or motivative appeals, or may even be testable in an *exclusively* subjective manner, it cannot be accepted as an answer to a *scientific* question" (p. 247, italics in the original). Yet Carnap makes the following comment about Feigl's interpretation: "At the present time I prefer not to emphasize the requirement of intersubjective confirmability as much as we used to do previously, but rather to consider it to be of secondary importance. I regard as meaningful for me whatever I can, in principle, confirm subjectively. This statement may be taken as a rough formulation of the *principle of empiricism*" (p. 882, italics in the original). If cognitive meaningfulness is relative to individual experience, is it not possible, as Feigl recognizes, for the metaphysician to claim that his statements are confirmable by his own experience and are thus meaningful to him? It is significant also that Carnap has proposed, as a solution to the debate over the admissibility of abstract terms in mathematics and

In order to avoid any possible misunderstanding, let me emphasize that I am not saying that Carnap and other spokesmen for the "standard position" in the philosophy of science have become historicists. Carnap himself takes note of one difference between the two positions when he observes that "historicism in its pure form" would reject his view "that in philosophy, no less than in science, there is the possibility of cumulative insight and therefore of progress in knowledge."[35] Perhaps we should speak of the later Carnap and those with similar views as "half-hearted positivists" and refer to their work not as "positivist" but as "positivistic." In any event, the decisive question is whether or not this decaying positivism, having conceded that logical reasoning is analytic and that experience does not provide a certain rock bottom of knowledge and meaning, can effectively oppose the more or less explicit historicism of writers such as Kuhn. I do not share my critics' confidence that it can.

Rudner summarizes his objections to what I say about the philosophy of science under five points,[36] to which I have replied as follows: (1) it is true that there is more going on in philosophy of science than my essay records, but I did not pretend to be giving a full account of all that has happened in the field; and none of the additional information supplied by Rudner invalidates my argument; (2) granted that the established viewpoint in philosophy of science has evolved under the impact of internal critiques, the issue is whether or not these changes have weakened its capacity to withstand historicism; (3) there may be good reasons for doubting my interpretations of Kuhn and Wittgenstein, but Rudner has not given any; (4) I have never claimed that the antipositivists have "defeated" the prevailing views in philosophy of science; (5) nor have I claimed that historicists have won a "victory" in the field.

In the second section of his critique, Rudner turns from "historical quarreling" to an exercise in "analysis." This exercise is scarcely worthy of serious attention; and I take notice of it only to point out some of Rudner's misrepresentations of my essay and the crudeness of his reasoning.

Rudner here concerns himself primarily with two arguments that he purports to find in my essay: (1) that the behavioral approach in political science has its epistemological roots in positivism; and (2) that the death of positivism entails the death of behavioralism. I do advance something akin to the first argument; and I shall return to it in a moment. The second argument, however, is Rudner's fabrication. In his initial formulation of this alleged argument, I am represented as saying that "the 'demise' of positivism means that the once flourishing 'behavioral approach' in political science is doomed."[37] This sounds, of course, like a prophecy that the destruction of behavioralism is imminent. I can show how flagrantly this suggestion misrepresents my position by quoting a passage from my essay:

> We have seen that while behavioralism continues to be the dominant approach in American political science, its epistemological foundations have been dealt a severe, if as yet unperceived, blow by critics of positivism. *It remains uncertain whether or not positivism, in some modified form, will continue to provide the guiding principles for political inquiry* or, if not, where the new epistemological and methodological principles will come from.[38]

It is conceivable to me that behavioral approaches will continue to predominate in political science for the foreseeable future (just as the "standard position" might continue to predominate in the philosophy of science) despite

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physics, a "principle of tolerance" according to which "everyone is free to choose the rules of his language and thereby his logic in any way he wishes" ("Intellectual Autobiography," p. 55). In the essay "Empiricism, Semantics, and Ontology" in *Semantics and the Philosophy of Language*, Leonard Linsky, ed., (Urbana: University of Illinois Press, 1952), pp. 208–228, Carnap holds that the choice of a linguistic framework, along with rules for framing statements and for testing, accepting, or rejecting them, is to be made on practical or pragmatic grounds, that is, according to whether the framework is expedient and fruitful for the purposes at hand. Here again, one must wonder if Carnap's later position is not so loose or liberal as to destroy any possibility of distinguishing between science and metaphysics on *epistemological* grounds.

Karl-Otto Apel concludes that Carnap's later position on language and fact comes very close to that of the Dilthey-Heidegger tradition. He interprets it as implying that "not even the facts of science are facts for the unchanging 'subject as such' (of "the language as such"), but they are constituted in a concrete and therefore historically determined human horizon of meanings." *Analytic Philosophy of Language and the Geisteswissenschaften*, p. 33. The favorable attention that Carnap is now receiving from continental thinkers may be due to his turn in their direction and not, as Braybrooke and Rosenberg suppose, a more conciliatory attitude on their part towards positivism.

[35] Rudolf Carnap, "Intellectual Autobiography," p. 41.

[36] Rudner, "On Evolving Standard Views," p. 841.

[37] Rudner, "On Evolving Standard Views," p. 827.

[38] "Positivism, Historicism, and Political Inquiry," p. 816. Emphasis added.

the weakness of their epistemological foundations. Later in his essay Rudner defines the problem as one of the logical rather than the historical connection between positivism and behavioralism. In restating my alleged argument, he invents a quotation to suit his purposes: "And now, finally we may turn our attention to the specific claim that the 'death' of positivism entails the 'death of behavioralism because behavioralism has its roots in positivist epistemology.' "[39] I make no such "claim" in my essay; and it is altogether unclear what "death" means in this context. I would argue that one who recognizes the theoretical defects of positivism must, if he follows the dictates of reason, question the soundness of "behavioral political science" as it is described in the textbooks, but I should think that many of the methods or techniques that have appeared under the aegis of behavioralism could be incorporated into epistemologically sounder approaches.

My essay asserts that the epistemological roots of the behavioral approach to political inquiry lie in positivism and, ultimately, in classical British empiricism. Rudner raises the perfectly valid question of whether or not this assertion is true. He never denies that it is true. He argues only that behavioralism does not presuppose positivism as I describe it.

I have begun to explore this issue elsewhere,[40] but I did not judge that a thematic treatment of it was necessary or possible in the present essay. Not content to raise the quite proper objection that I do not give detailed support for my assertion about the epistemological roots of behavioralism, Rudner goes on to construct—sometimes using only scattered fragments of what I say and report—a view both of positivism and behavioralism which he then identifies as *my* view.

The view of positivism is constructed from a paragraph in which I meant only to characterize empiricist-positivist epistemology as it has been developed by such writers as Hume, Mach, the logical atomists, and the founders of the Vienna Circle. It should be clear that the paragraph was not intended as a complete account of positivism. One can easily think of exceptions to what I call a typical positivist outlook, especially if he considers the development (or decay) of logical positivism after it abandoned some of the original doctrines of the Vienna Circle. Rudner's lengthy analysis of the paragraph indicates that it is in need of a minor amendment at one point. The sentence that he designates as (e) might better read: "In order to be meaningful, our statements about the external world must be tested *or testable* against the data of experience." This amendment would accommodate the weaker forms of verification that came to be favored in the course of logical positivism's evolution. In fact, the final sentence of the paragraph, which Rudner fails to quote, speaks of the loose form of the verifiability principle. As for the rest of Rudner's analysis, it amounts for the most part to evasion and quibbling.

The careful reader will have noticed that one of Rudner's techniques of analysis is to reduce a sentence to absurdity by giving it a tortured interpretation or even by changing some of its words. Rudner uses both devices in analysing my statement that a critical task for positivist epistemology, given the empiricist-positivist view of science, is to uphold "the assumption that our generalizations about the external world are reliable if and only if they are constructed from or tested by the raw material of experience." The issue here is not that of deciding when our statements are "meaningful," as in the sentence considered above. Instead, it is one of deciding when our generalizations about the external world can be accepted as a reliable or dependable part of the corpus of scientific knowledge. The clear implication of my term "tested" is that such generalizations must have *withstood* empirical testing in order to be accepted as reliable. I grant that contemporary positivists would not regard success here as a sufficient condition of reliability. Rudner converts the statement into a "logical howler" by substituting the term "testability" for "tested." According to the criterion of "testability" which Carnap developed after his "turn," "[a] sentence which is confirmable by possible observable events is, moreover, testable if a method can be specified for producing such events at will . . ."[41] This means that a sentence need not have actually withstood empirical testing in order to be regarded as significant or meaningful. It is only necessary that there be observation sentences that can contribute *either positively or negatively* to its confirmation. Both a generalization and its contradictory or its contrary may, as Rudner says, be *testable* and thus significant or meaningful, but it is not the case that both can have been *tested* or have withstood empirical testing, and been found reliable. The logical difficulty which Rudner pretends to find in my statement is entirely one of his own creation. Rudner's other objection to the statement under consideration is that according to the "standard conception," "only theories *as wholes,* not single (theoretical) generalizations, are taken to be testable . . ."[42] My statement speaks, however, of "generalizations," which can mean *sets* and *systems* of generalizations, or theories as wholes, as it can mean *single* generalizations. The other problems which Rudner raises about my discussion of positivism can, like these, be resolved by a fair consideration of the issues.

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[39] Rudner, "On Evolving Standard Views," p. 844.

[40] See "Hume's Contribution to Behavioral Science," *Journal of the History of the Behavioral Sciences,* 12 (April, 1971), 154–168.

[41] Rudolf Carnap, "Intellectual Autobiography," p. 59.

Rudner constructs my alleged view of behavioralism not from what I say about it but from what I report about the *postbehavioral* positions. He selects out snippets from my descriptions of various postbehavioral critiques and asserts, without offering a shred of evidence, that "[t]hese citations help us to infer what Miller takes behavioralism to be."[43] As for the postbehavioral critiques themselves, Rudner rejects them after raising objections to the snippets that he draws from my presentation, never bothering to look at the original sources. The sad truth is that Rudner seems actually to believe that he has vindicated behavioralism by this incredible exercise. One might say that what Rudner gives us in the second section of his critique is an inadvertent caricature of the method of logical analysis itself.

Response to Landau

Professor Landau agrees with much that I set out to establish in my essay. It is true that he expresses some reservations about the connection that I presuppose between behavioralism and logical positivism.[44] He dislikes my term "historicism," which he would replace by "epistemological relativism." Yet despite these reservations, he goes on to describe as "rather evident" my thesis "that the recent antibehavioral movement is an expression of historicism; that it not only opposes the methodology of behavioralism but rejects its epistemology (positivist) as well."[45] He recognizes that Kuhn's position, like similar antipositivist arguments in the philosophy of science, may properly be described as "historical relativism." Above all, he and I agree that historical relativism is untenable and that it cannot provide a sound epistemological basis for political inquiry.

There are, nonetheless, some genuine points of disagreement between us. First, he thinks it patently false that historicism has replaced positivism as our leading theory of knowledge. I would call attention here to my reply to a similar criticism by the other symposiasts. Landau admits, as they do, that the original principles of logical positivism have been largely repudiated, but he is reluctant to take this as evidence of the movement's decline. Furthermore, he underestimates, as they do, the force and influence of historicism in modern thought. A second point of disagreement concerns the most suitable grounds for opposing historicism. Landau seems to believe that a viable alternative to historicism can be established on what are essentially positivistic grounds, while I would argue that the search for an alternative requires us to break fundamentally with positivistic theories of knowledge, especially as regards the nature and limits of reason.

Landau uses his critique of my essay for a constructive purpose; and I wish to use my response in the same way. I shall attempt to clarify my usage of the term "historicism," indicate why his refutation of the relativist position does not have force against historicism in its most powerful form, and, finally, suggest how a viable alternative to historicism might be constructed.

The practice of designating modes of thought by general terms ending with "ism" must be counted among the necessary evils of academic discourse. Though subject to abuse, these terms seem, nonetheless, to be indispensable for the discussion of broad issues. Even the critic who questions an author's use of them can scarcely avoid introducing others of his own in discussing the same topics. So it is in the present symposium. Landau, for example, criticizes my use of "historicism," which he describes as a "soft" and "noisy" term that fosters oversimplification. In order to designate the positions in question, he introduces the term "relativism," along with such variants as "epistemological relativism" and "historical relativism." One may wonder if these terms are harder or more muted than "historicism."

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[42] Rudner, "On Evolving Standard Views," p. 842.

[43] Rudner, "On Evolving Standard Views," p. 841.

[44] What my essay says is that the epistemological roots of the behavioral approach "lie in positivism and, ultimately, in classical British empiricism." Inasmuch as there has been strong disagreement within this tradition on the respective roles of reasoning and experience in scientific method, a disagreement among behavioralists on this matter cannot be taken as evidence that the disputing parties do not have a common indebtedness to the empiricist-positivist tradition. Landau suggests that innovations in research design and statistical techniques were more important for the rise of behavioralism than the logical positivists' writings about the nature of science. It must be remembered, however, that these techniques embody presuppositions about human knowledge that are elaborated and justified in the empiricist-positivist tradition. Furthermore, it was chiefly to writings in the logical positivist tradition that behavioralists turned in attempting to understand and explain what science is.

[45] Martin Landau, "On Objectivity: Comment on 'Positivism, Historicism, and Political Inquiry'," *American Political Science Review,* 66 (September, 1972), p. 846.

I chose the term "historicism" with some hesitation, for while European scholars have commonly used it to designate the epistemological principles which I discuss, American scholars are often unfamiliar with this meaning. I am prepared to defend it as an appropriate term, but I am willing also to accept any substitute—and my essay mentions several, including "relativism"—that will facilitate discussion of the substantive epistemological viewpoint in question.

The term "historicism" at least has the merit of reminding us that modern relativism is the consequence of the increasing emphasis, after the eighteenth century, on the historical or temporal character of all things, including man and nature. This emphasis has led to the ontological thesis that reality has the character of process or becoming. It has led to the epistemological thesis that man, the knower, must always view the world from a temporal or historical perspective. Earlier views of knowledge had held that reason and experience enable us to transcend the limitations of time and place and to discover permanent truths about the nature of things, including political things. By the end of the eighteenth century, the powers assigned to reason had been sharply reduced by philosophers in the empiricist tradition, while the role of experience in our knowledge of nature had been greatly elevated. Hume, for example, developed the thesis, later to find expression in logical positivism, that reason, in itself, cannot tell us anything about the world external to the mind. Our knowledge of external objects and their relationships arises from experience in accordance with regular principles of the human understanding. During the century or so between Hume and Nietzsche, a radical or relativistic historicism emerged through stages that I summarize in my essay. Writers in the first stage, most notably Kant, called into question the empiricist teaching about the directness or immediacy of experiential knowledge without, however, restoring reason's capacity to view the world of nature as it is in itself. The mind was said to have access to experience only as it is transformed by categories of thought. In the second stage, Hegel and others taught that the presuppositions and categories of thought have varied from one historical epoch to another. This teaching did not lead to epistemological relativism so long as philosophers affirmed that history itself is an intelligible whole, which develops according to intelligible laws that are accessible to reason in the final or culminating epoch of world history.[46] Yet once it became doubtful that the historical process is intelligible in itself or progressive in the insights that it yields, intrepid thinkers such as Nietzsche concluded that if the principle of the historicity of the mind is correct, all truths must be relative to time and place.

Landau asks, quite properly, just which of these principles must be adopted by a thinker in order for him to be regarded as an historicist. The term "historicism" is commonly applied, first of all, to positions such as Hegel's that affirm the historicity of the mind, even though such positions avoid relativism by holding that the philosopher can, in the culmination of history, grasp principles or laws about the development of history as a whole that are final and not to be superseded. This position, which has been called "theoretical" or "contemplative" historicism, should be distinguished from the "radical" or "existentialist" historicism that came later. Radical historicism rejects the possibility of final knowledge of nature or history and embraces relativism. This type of historicism, in its implicit and explicit forms, is the main concern of my essay. Landau seems to have in mind this radical form of historicism, or at least Mannheim's version of it, when he refers to "historical relativism."[47]

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[46] My own understanding of this "theoretical" or "contemplative" historicism has been greatly enhanced by reading Alexandre Kojève's *Introduction to the Reading of Hegel,* edited by Allan Bloom and trans. by James H. Nichols, Jr. (New York: Basic Books, 1969). In the "Editor's Introduction," Bloom says of Kojève: "From him one can learn both the implications and the necessary presuppositions of historicist philosophy" (p. viii). Bloom here comments on Kojève's relationship to Hegel, Marx, and Heidegger. Kojève is among a number of proponents or friends of Marxism who oppose epistemological relativism (see especially pp. 100–149 of *Introduction to the Reading of Hegel*). For a Marxist theory of cognition that tends toward relativism, see Leszek Kolakowski, "Karl Marx and the Classical Definition of Truth," in *Toward a Marxist Humanism,* trans. by Jane Z. Peel (New York: Grove Press, 1968), pp. 38–66. The tension within the Marxist tradition between theoretical and radical historicism is of utmost importance for our present inquiry.

[47] In assessing an author's statements about historicism, it is essential that one be clear about the meaning that the author attaches to the term. At the conclusion of an important new study of nineteenth-century thought—Maurice Mandelbaum's *History, Man, and Reason* (Baltimore: Johns Hopkins Press, 1971)—the author states that historicism "has been abandoned" in our time (p. 371). Closer inspection shows, however, that he means something akin to what I have called "theoretical" or "contemplative" historicism, as the following passage suggests: "Historicism is the belief that an adequate understanding of the nature of any

Landau develops several arguments against historical relativism which, he believes, are adequate to refute it. First, he offers the familiar argument that the principle of the relativity of all truth invalidates itself when it is put forth as a true principle. Although some formulations of historicism might be self-invalidating in this manner, this is less obviously true of the radical historicism of writers such as Nietzsche and Heidegger, who recognize the absurdity of proposing relativism as a theoretical thesis or as an empirical generalization. The radical historicist would not claim that his worldview is truer, in an objective sense, than other worldviews or valid for all historical epochs. He might speak of his relativistic worldview as his "project" or creation, which he has appropriated as his personal truth by his free act of choosing it. Again, he might regard it as a gift vouchsafed by fate to modern man, a gift which men of our historical epoch are free either to accept in anguish or to reject at the risk of ultimate meaninglessness and despair.

Landau would here object that radical historicism abandons the very possibility of holding assertions accountable. The historicist, in turn, might offer his own definition of accountability. Accountability, he might suggest, resides in the individual's willingness to choose resolutely among competing worldviews, recognizing that ultimately no empirical or rational justification exists for his choice and that he must accept full responsibility for its personal and social consequences.

The historicist might argue, further, that Landau fails to provide compelling support for his definition of accountability. Landau's principle of accountability rests on the argument that men are never complete captives of a prevailing conceptual scheme. We have the capacity to be surprised, to recognize anomalies and novelties. Few historicists would insist, however, that men are completely culture-bound or fully determined socially, such that those who share a common worldview must agree with prevailing opinions in every respect. In fact, individualistic historicists, such as Nietzsche, would hold that exceptional men can question even the most general presuppositions of their epoch or culture and create new worldviews. The real issue, then, is not whether men can recognize discrepancies between their conceptual schemes and experience or whether conceptual innovation is possible. It is whether there are grounds for believing that the substitution of one conceptual scheme for another can represent progress toward a true understanding of the world and not mere change.

Insofar as this central issue is concerned, Landau gives no indication of wishing to restore the older view that reason can go beyond the manipulation of symbols to gain its own true vision of the nature of things. He declares, with respect to the possibility of objective truth, that "the prime test is the test of experience." Yet he is willing to "admit out of hand that there is no 'immaculate perception.' "[48] Observation is conditioned by values; and values themselves cannot be held accountable, since they belong to the noncognitive or emotive realm. In the final analysis, Landau's principle of accountability seems to rest not so much on the veracity of experience as on the benefits to be gained from "mutual rational control by critical discussion." In the absence of systematic criticism, "there is only change; when it constitutes an institutionalized procedure, there is progress."[49] Landau offers no *epistemological* arguments, however, for the belief that systematic criticism produces progress towards knowledge of reality and not merely the substitution of one conventional and ultimately unsupportable theory or conceptual framework for another. Why should we not conclude that systematic discussion leads merely to the replacement of individual error by collective error?

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phenomenon and an adequate assessment of its value are to be gained through considering it in terms of the place which it occupied and the role which it played within a process of development" (p. 42). My essay agrees, of course, that historicism in this sense has been largely repudiated. On the other hand, Mandelbaum agrees with my argument that the rebellion against reason associated with writers such as Nietzsche (Mandelbaum calls their view "radical voluntarism") "has subsequently become a powerful intellectual influence on our time" (p. 364). Mandelbaum and I disagree as to whether or not the term "historicism" can properly be applied to the thought of Nietzsche and to the existentialism of our century. I would say in defense of my usage of the term that these latter views, no less than the earlier "contemplative" historicism," arose from the insistence that man must be understood as a radically historical being. Another usage of "historicism" that contrasts with my own can be found in Glenn Tinder, "The Necessity of Historicism," *American Political Science Review*, 55 (September, 1961), pp. 560–565. Tinder defines historicism as "the ascription to history of an overall direction and goal" (p. 560). He concludes that since "a rationally demonstrable theory of history is impossible," "the final basis of any particular philosophy of history must be faith" (p. 563).

[48] Landau, "On Objectivity," p. 851. Nietzsche uses the term "immaculate perception" (*unbefleckten Erkenntnis*) in *Thus Spoke Zarathustra* to disparage traditional views of knowledge. See Walter Kaufmann, ed., *The Portable Nietzsche* (New York: Viking Press, 1954), pp. 233–236. I do not know if Landau is aware of Nietzsche's use of this term or if he is prepared to accept the historical connotations that Nietzsche gives it.

[49] Landau, "On Objectivity," p. 855.

Landau does give a practical or pragmatic justification for the methodology of natural science: Natural science and its technology "work." It is not likely, however, that this justification would be convincing to the historicist, who would point out, first, that positivistic *social* science can scarcely be upheld by its practical achievements and, second, that the crises of modern life have made it doubtful that even natural science "works" in the sense of contributing to man's ultimate well-being.

So might run an historicist's reply to Landau's essay. I confess some uncertainty as to whether or not this reply does justice to Landau's position. It is true that he makes statements about the character of reason and experience that seem to cut the ground from under his insistence on scientific objectivity. Yet at the same time, one finds indications in Landau's essay of a wholesome, common-sense view of empirical knowledge that is unaffected by positivism and historicism. In several places, he suggests that observation makes us aware of things or objects that exist in the world. The purpose of science is to discover what these things are or to what category or class they should be assigned. Our conception of what a thing is (the intention or connotation of our concept) is reliable only if we can determine, from observation, that the thing exhibits the common properties that our conception leads us to expect of it. There must be a symmetry or correspondence between our concepts and the things themselves. Landau describes the assignment of an object to a class in the following terms:

> If one says that object A is a legislature and this (class) term has been provided with clear criteria of membership, then one expects to find (predicts) that A exhibits these properties. If it does not, and we can only find this by observation, it cannot be assigned to this class. Assignment thus is only expressible as a hypothesis.[50]

As this statement shows, Landau assumes that there are, among the things in the world, political things to which the concepts of political theory must somehow correspond if that theory is to be acceptable.

I believe that one could formulate a view of knowledge that avoids the difficulties of positivism and historicism by following out rigorously and completely the implications of these common-sense assumptions about what we know. If we know things as things, their "whatness" or "class," through observation, then the content of empirical knowledge is not reducible to mere sense data, nor is "thing-language" a mere convenience that we can just as easily dispense with. Things have an essential identity and intelligibility in themselves. They are not mere constructs of the mind. Their meaning is discovered and not created. Observations will thus have a core of agreement from one epoch or culture to another.

These reflections on ordinary experience would require one to amend Landau's position at some points. It is obvious that we do not see political things, such as legislatures, in the same way that we see objects in nature, such as trees, or physical artifacts, such as chairs and beds. Observation is not sufficient, then, to determine what political things, such as legislatures, really are or to what class they should be assigned. Someone could observe every tangible aspect of the United States Congress without "seeing" a legislature. Political things come to sight in what is said about them, that is, in opinion; and they are visible not to the physical eye but to the eye of the mind. In order to reach clarity about the nature of political things, there must (to use Landau's words) be "mutual rational control by critical discussion." The model for such discussion, however, is not what empiricists have said about the proliferation of alternative theories but what the ancients said about dialectical reasoning, whose purpose was to move from opinions about things to knowledge of their true natures.[51]

Moreover, it is not the case that standards of evaluation belong to a different sphere from "facts," that is, to a "noncognitive" sphere. When we understand the "whatness" of a thing, its essence or nature, we apprehend, at the same time, criteria for evaluating all things that belong to the same class. For example, the general conception of a legislature must include some indication of the purpose of a legislature and, therefore, a standard for judging the adequacy of any legislature that might exist. Landau's essay leaves us with an unresolved difficulty: our knowledge of things cannot be "neutral" or "value-free"; but if values are "noncognitive," our value-impregnated knowledge of facts would seem to be inescapably subjective and relative. This difficulty disappears when it is recognized that the "ought" is no less a matter of reliable cognition than the "is."[52]

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[50] Landau, "On Objectivity," p. 853.

[51] An urgent task for those concerned with the epistemological foundations of political inquiry is the exploration of what it means to "see" political things. Among the sources that I have found helpful in this regard are the following: Hans Jonas, *The Phenomenon of Life* (New York: Harper & Row, 1966); Robert Efron, "What is Perception," *Boston Studies in the Philosophy of Science*, IV, 137–173; Stanley Rosen, *Nihilism*, pp. 140–235; Leo Strauss, *Natural Right and History* (Chicago: University of Chicago Press, 1953), pp. 120–164; Jacob Klein, *A Commentary on Plato's Meno* (Chapel Hill: University of North Carolina Press, 1965), pp. 108–172; Walter Berns, "The Behavioral Sciences and the Study of Political Things: The Case of Christian Bay's *The Structure of Freedom*," *American Political Science Review*, 55 (September, 1961), pp. 550–559; and Herman L. Sinaiko, *Love, Knowledge and Discourse in Plato* (Chicago: University of Chicago Press, 1965), pp. 119–190.

In searching for an alternative to historicist theories of knowledge, one must remember that the problem of knowing is but the other side of the problem of being. Reliable knowledge is not possible unless there is something constant and intelligible to be known. The great power of Heidegger's thought arises from his success in developing the ontology, the account of "to be," that is implicit in the assertion that all thought is radically historical or even in the assertion that the totality of what is known "is like a ship for which there is no dry dock and which therefore has to be repaired and rebuilt while floating on the open ocean."[53] Heidegger opposes the view of traditional philosophy that "to be" in the highest sense means "to be always" or to be an intelligible object of thought. What philosophers have regarded as a permanent and intelligible whole is said by him to be changing in an unpredictable way, such that the modes of its presentation to human thought in the future cannot be anticipated. Logical positivists and their heirs are blind to the problem of being and thus to the force of Heidegger's position, because they think that Carnap, through some analytical legerdemain, succeeded in eliminating metaphysics.

Philosophical inquiry can nourish political science by giving an account of knowing and being that favors the quest for knowledge of the nature of political things and of the right, or the good, political order. The account of knowing must restore to reason the capacity to go beyond the perspectival ambiguities of sense perception and opinion to a noetic vision of things as they are. Modern empiricism, having denied to reason any power in itself to know the world, found that it could not resist historicism by appealing to the authority of experience, especially after it conceded that there is no indubitable knowledge of the immediately given or that experience is conditioned irretrievably by historical forces. The account of being must reflect our awareness that the whole consists of definite kinds or classes of things. It must restore the natural as something permanent and intelligible and also as something high and noble. The modern conception of nature as something less than human finds expression in both positivist and historicist approaches to the study of man and society. Positivism attempts to understand man as part of nature; but since it denies that an essential or qualitative difference can be found in nature between the human and the subhuman,[54] it tends to reduce the human things to the infrahuman or to the physical. Historicism, by contrast, concludes that if the natural is less than human, man must be understood in light of his history and not his nature; but this conclusion leads finally to the denial of any permanent ground of evaluation and explanation.

Conclusion

I can conceive of two lines of attack by which a thoughtful critic might attempt to refute the general thesis of "Positivism, Historicism, and Political Inquiry": he might argue that I have not correctly identified the major contending positions in epistemological debate over the past century and their relative strength, or, conceding that this part of my thesis is sound, he might argue that this epistemological debate does not find expression in political science in the way that I have described. The first line of attack would require detailed attention to post-Kantian theories of knowledge, while the second would require detailed attention to developments in political science.

Neither line of attack is pursued in a sustained fashion by any of my fellow symposiasts. Each directs his critique mainly against what I say about recent developments in the philosophy of science, where only a small part of the relevant epistemological debate has occurred. Even if my observations about this subfield of philosophy were wrong, my broader thesis about developments in modern epistemology generally and in political science could still be correct. I do not believe that my critics have succeeded in showing that what I say about the philosophy of science is wrong, but I am quite willing to leave the final judgment in this matter to the attentive reader who has examined carefully the several parts of this symposium.

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[52] Compare Harry V. Jaffa, *Equality and Liberty* (New York: Oxford University Press, 1967), pp. 190–211.

[53] Rudolf Carnap, "Intellectual Autobiography," p. 57.

[54] Carnap declares that "[a]ll empiricists have abandoned the earlier belief that there is a fundamental difference, a 'difference in kind', between man and the other animals, and between organisms and the inorganic world. Nobody denies that there are differences and that they are of very great importance both theoretically and practically. But these differences have no sharp boundary lines; they are differences of degree within a continuum. It is possible, of course, to draw a line by definition between human beings and other animals; but any such line is to some extent arbitrary, that is to say, the line might be drawn with just as good reasons somewhat later or somewhat earlier." *The Philosophy of Rudolf Carnap*, p. 884.

Collective Goods and Future International Collaboration*

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There appears to be emerging among students of highly industrialized societies a consensus that a fundamental institutional transformation is under way, that signs of a "postindustrial,"[1] if not "postmodern," order are becoming evident. Views differ less on whether these changes are occurring, than on their magnitude and their desirability. Two fundamental changes, concerning the *kinds of goods* which are predominantly produced by these societies, and the *manner in which* these goods are produced, are foreseen. First, the postindustrial society is characterized as one which has become preoccupied with the production of services, research and development, education and knowledge, and various social amenities—publicly relevant goods, in sum. Second, it is characterized as a society in which these goods are produced and/or purchased communally—in which public decisions and planning will have replaced the market "as the arbiter of various social and economic choices."[2]

The growth of the public sector, resulting from attempts to regulate market irregularities, to provide social welfare which the market failed to provide, and to support enterprises which otherwise would not exist, is part of the tradition of the advanced industrial societies.[3] But it is argued today that a qualitative change, of major dimension, is being effected by the impact of science and technology upon these societies. In a growing, highly uneven, and often mystifying literature, science and technology are portrayed as offering possibilities and posing problems of such a nature that the most characteristic institutions of the industrial order —whether the mature corporation,[4] the market economy,[5] bureaucratic forms of organization,[6] or representative government[7]—find themselves unable to exploit fully the possibilities, and unable to deal adequately with the problems. Basic organizational changes are said to follow from attempts to respond to this predicament.

Much the same kind of reasoning is exhibited by students of international organization: the incidence and the character of international organization is changing, they argue, as states find themselves incapable of fully exploiting the possibilities offered by, and of adequately coping with the problems posed by, developments in science and technology. For example, industrial efficiency and economic growth, military security and the ability to play a major role in world affairs, are all said to have become dependent upon leadership in a number of research-intensive sciences. Nations lacking sufficient national resources to maintain high levels of research and development, yet wishing to enjoy its benefits, must look to their neighbors for cooperative production arrangements. As a French Minister of Science has declared: "Europe will be made by the atom, space, aeronautical construction, and computers, or it will not be made."[8] Moreover, coping with the unforeseen and unanticipated consequences of past, present and future technological developments is said to require the systematic exchange of information among states, as well as the monitoring, coordination and harmonization of national behavior. In addition, within the next quarter century, technologies will offer a number of new capabilities, such as that of environmental alteration, including the modification of weather and climate conditions, the fuller exploitation of the seabed, the developing of new sources of energy and of new modes of communication and transportation. As a result of the problems posed by these developments, students of international organization have predicted that "nation-states will have to accept a degree of international regulation and control over their nominally domestic activities that goes well beyond the situation today."[9] In sum, just as the kinds of goods and the manner in which they are coming to be produced is said to be changing domestically, so it is internationally: greater amounts of joint services and joint production, and a greater degree of joint regulation of national activities will be required.

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[1] Daniel Bell popularized this term in his "Notes on the postindustrial society," *The Public Interest,* No. 6 (Winter 1967), pp. 24–35; No. 7 (Spring 1967), pp. 102–118.

[2] Bell, p. 103.

[3] For an extended, informed and informative analysis of these developments, consult Andrew Shonfield, *Modern Capitalism: The Changing Balance of Public and Private Power* (London: Oxford University Press, 1965).

[4] John Kenneth Galbraith argues that the mature corporation is being fundamentally transformed, especially in its relations with the social system around it, as a result of the rise of the "technostructure" in turn brought about by the impact of science and technology. *The New Industrial State* (New York: Signet Books, 1968).

[5] Robert L. Heilbroner contends that the market economy is being fundamentally transformed, as a result of ever greater reliance upon planning, in turn generated by the impact of science and technology. *The Limits of American Capitalism* (New York: Harper & Row, 1966), Part II.

[6] As a result of the general uncertainties and complexities generated by modern sciences and technologies, which traditional organizational structures have difficulty coping with, we are said to be heading "beyond bureaucracy," by, among others, Warren Bennis and Philip Slater (in *The Temporary Society* [New York: Harper & Row, 1968]).

[7] The classic work of this genre is Don Price's study of the impact of public science funding, and of advisory structures of various kinds, upon American government. *The Scientific Estate* (New York: Oxford University Press, 1965).

But what does it mean to say that these will be required, or that these will have to be accepted? Are there technological imperatives to which states will respond?[10] Are there human imperatives to which states will respond?[11] Are there general environmental conditions, which are exacerbated by the impact of science and technology, to which states will respond?[12] What kind of world order will these responses of states generate? And what will be the consequences for political life, both national and international, of whatever world order is in fact generated?

In attempting to deal with these questions, we might employ any number of available metaphors or approaches, some of which are briefly assessed in the next section. To anticipate the argument, the formulation I will propose, and which is demonstrated and illustrated below, poses the basic problem of international organization as one of national *choice under constraints:* that is, given the structure of the contemporary interstate system, what are the general conditions under which states, with differing objectives and different capabilities, choose to collaborate with others? How do developments in various sciences and technologies affect those general conditions? And what are the consequences of various kinds and levels of collaboration states choose?

Functionalism and International Organization

Among the most powerful of metaphors available to us in the study of international organization is one or another variant of "functionalist" thinking, or the belief that somehow specialized structures will evolve to perform new tasks or to fulfil new needs as these arise.[13] The most general and most ambitious variant is that which actually postulates the existence of evolutionary trends: that in reacting to, and adapting to, its environment, a people will build for itself ever higher forms of sociopolitical organization, from tribes to baronies to global systems:

> The long-run trend toward integration seems to be for functions, authority and loyalties to be transferred from smaller units to larger ones; from states to federations; from federations to supranational unions; and from these to super-systems.[14]

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[8] Cited in Robert Gilpin, *France in the Age of the Scientific State* (Princeton: Princeton University Press, 1968), p. 416.

[9] Eugene B. Skolnikoff, *The International Imperatives of Technology* (Berkeley: Institute of International Studies, Research Series, No. 16, 1972), p. 153.

[10] The most extensive compendium of such "imperatives" is Skolnikoff's *The International Imperatives of Technology;* see also his "The International Functional Implications of Future Technology," prepared for delivery at the 66th Annual Meeting, American Political Science Association, Los Angeles, September 8–12, 1970.

[11] See, for example, Richard A. Falk, *This Endangered Planet* (New York: Random House, 1971).

[12] Among the general environmental conditions which have received attention of late is that of interdependence. Much of the political science work with the concept was stimulated by Richard N. Cooper's *The Economics of Interdependence* (New York: McGraw-Hill, 1968). In Cooper's usage, the term is only loosely defined; and the political science work based on Cooper's formulation has, thus far, simply drawn our attention to the phenomenon. Among the better discussions are Oran R. Young, "Interdependencies in World Politics," *International Journal,* 24 (Autumn 1969), 726–750; and Edward R. Morse, "The Transformation of Foreign Policies: Modernization, Interdependence, and Externalization," *World Politics,* 22 (April 1970), 371–393. The extent to which particular situations might increase the level of interdependence between states, or effect interdependencies of a new kind, is only loosely explored at best.

[13] An excellent discussion of functionalist thinking in the context of international organization may be found in James Patrick Sewell's *Functionalism and World Politics* (Princeton: Princeton University Press, 1966).

[14] This kind of argument is quite common. The particular citation is from Amitai Etzioni, "The Dialectics of Supranational Unification," *International Political Communities* (Garden City: Anchor Books, 1966), p. 147.

We might, then, simply correlate levels of technological development with levels of sociopolitical organization, and go on to postulate the emergence of supranational forms.

Albeit tempting as an intellectual shortcut, this variant of the metaphor is quite deceptive. First, it overemphasizes linear continuity, at the expense of crucial differences and discontinuities between forms of social organization. Second, it offers precious little insight to the scholar concerned with understanding and linking together the many discrete activities which such historical sweeps subsume and distort.[15] Finally, it offers no guidance whatever to political actors, through whose everyday lives historical evolution is presumably expressed.

Alternatively, one might adopt a less pretentious variant of the same metaphor, that from an initial need specific organizational responses will follow:

> Given the initial need and an operation of technical self-determination, the individual functional units will develop.[16]

Applying these notions to the functional area before us, we might conclude that:

1. All technologies in the long run have global impacts of varying intensities as a consequence of their diffusion.
2. Some technologies possess a stronger international logic than others because successful research and application are beyond the effective capacity of any single nation-state.[17]

Hence, a certain level of cooperation and collaboration will follow. The difficulty with this formulation is that it is essentially apolitical, relying, as it does, upon the "logic" of technology. And the step which is presumably of greatest interest to political scientists and most crucial for political actors, namely the manner in which initial needs become expressed in organizational results, "is treated as a residual consideration."[18]

Finally, the least presumptive variant of this mode of thinking might be employed, that of "neofunctionalism," particularly since it constitutes the best and the most elaborate body of theory about the processes of international organization. Thus—using an early formulation for illustrative purposes—the expectations and aims of various political actors within states in the functional area of science and technology might be investigated. Will such actors favor the international regulation of problems associated with technological developments? Will they be opposed? Or will they exhibit "convergent expectations," such purely instrumental and short-run aims which can, by "creative compromise," be translated into support for integrative policies and institutions? And do possibilities of task-expansion and spillover attend these policies and institutions?[19]

Neofunctionalism too has been criticized on a number of grounds; for our purposes here two aspects of the approach represent important constraints. First, by relying upon functions and functional contexts as major explanatory variables, one tends to attribute an independent existence to them:

> Decisions made by identical officials, in organizations with a stable membership, in a non-revolutionary socio-ideological setting with similar institutional characteristics, nevertheless vary sharply, in terms of their integrative impact, *depending on the functional context.*[20]

Such an attribution obscures the fact that functional contexts do not exist *apart from* a particular configuration of actor attributes in relation to any given issue: different actors' differing objectives, pursued with unequally distributed resources, define "functional" contexts. And scientific or technological developments affect international organization only to the extent that they affect such a configuration.

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[15] Note, for example, the refutation by Karl W. Deutsch and his collaborators, in *Political Community and the North Atlantic Area* (Princeton: Princeton University Press, 1957): "In this view, as villages in the past have joined to make provinces, and provinces to make kingdoms, so contemporary states are expected to join in ever-larger states or federations. . . . Our findings do not support this view" (p. 24).

[16] Sewell, *Functionalism*, p. 12.

[17] Edward Miles, "Relationships between Technology and Intergovernmental Cooperation in International Organization," Prepared for the Conference on Functionalism and the Changing Political System, Bellagio, November 20–24, 1969. Miles appears to be summarizing functionalist thought and not necessarily expressing his own theoretical preferences. The citation is from pp. 1–2.

[18] Sewell, *Functionalism*, p. 9. A related line of reasoning, containing elements of both this and the previous variant of the basic metaphor, argues that *because* the modern state is outmoded and ill-suited as an organizational form in the face of the interdependencies and the complexities of contemporary political life, particularly those generated by the impact of technologies, *therefore* new transnational or supranational systems will arise. (See, for example, Louis Armand and Michel Drancourt, *The European Challenge* [New Work: Atheneum, 1970].)

[19] This illustration, which obviously does not do justice to the subtlety of the neofunctionalist argument and research, is drawn from Ernst Haas's early work on European integration. (For a summary statement, see Haas, "The Challenge of Regionalism," *International Organization*, 12 [Autumn, 1958], 440–459.) The most recent and most extensive presentation of neofunctionalist thinking and re-evaluation is contained in *Regional Integration: Theory and Research*, ed. Leon N. Lindberg and Stuart A. Scheingold, *International Organization*, 24, No. 4 (Autumn, 1970).

[20] Ernst B. Haas, "International Integration—The European and the Universal Process," *International Political Communities*, pp. 103–104; emphases added.

Second, the very concern with *integration*—while having obvious relevance to ongoing political processes, particularly in Western Europe —has tended to obscure the fact that integration is itself the limiting case of a more general phenomenon, and that questions about integration are themselves a subset of a more general set of questions.[21] Briefly, these are: given the structure of the contemporary interstate system, when, where, and how do states seek to organize activities internationally? And what particular mode of organization—coordination, collaboration, integration—is selected under what conditions? They are, then, a subset of questions about *international organization* in general. It is this more general phenomenon that I propose to analyze here.

In sum, I too am concerned with new forms of sociopolitical organization, with new international arrangements and policies, and with new aims and expectations national actors may come to hold; but I wish to make no assumptions about these evolving from new environments, following from new and inherently international needs, or being generated by new functional contexts enjoying an independent existence. Given this perspective, the problem becomes one of determining why and how states choose from among alternate modes of organizing activities, both national and international, given certain possibilities and constraints; and of suggesting the consequences thereof, for the manner in which political life is organized both nationally and internationally.

Choice, Constraints, and the Propensity for International Organization

The task for students of international organization is to specify when, where, and how states seek to organize activities internationally. Scientific and technological developments will affect international organization only to the extent that they affect a particular configuration of actor objectives and capabilities, vis-à-vis a particular issue. Hence, we must first inquire into the basic dynamics of international organization in the contemporary interstate system. Only then can we speculate about the likely impact of scientific and technological developments.

My purpose here will be to demonstrate and to illustrate one possible theoretical system allowing the exploration of these questions. In order to keep the discussion manageable, the focus throughout will be limited to an individual actor's changing propensity to organize activities internationally, as that actor faces different possibilities and constraints. The analysis is based upon certain assumptions about the structure of the contemporary interstate system and about states as actors within that system; these must first be explicated.

The contemporary interstate system is here viewed as a modified Westphalia system.[22] In theory, since the Peace of Westphalia the interstate system has been a decentralized one: states are subject to no external earthly authority, and there exists no organization *above* states, only *between* them. The Westphalia system consists of a multiplicity of independent states, each sovereign within its territory, and each equal to every other. It recognizes only one organizing principle, the will of states, thereby giving the collective decision-making system its decentralized character. In practice, the Westphalia system has become partially but progressively modified: spheres of influence modify the principle of equality; supranational actors modify the principle of no external earthly superior authority; an ever more complex pattern of interconnectedness of decisions, events, and developments modifies the principle of independence. And to the extent to which states subsequently "will" collective principles and forms of decision making, the decentralized character of the system is itself modified—much as the market economy is modified by governmental intervention and regulation.[23] In fact, one can sensibly speak of an interstate system only insofar as the international organization of activities exists, however informal or minimal it may be.

Thus, I view the contemporary interstate system as a partially modified Westphalia system: still essentially decentralized, and based upon the will of states, but with each state willing to accept and to engage in some form and some extent of international organization. An understanding of the basic dynamics of international organization, then, entails the determination of when states will seek to organize activities by means of informal arrangements and when by institutionalized; when the arrangements will concern simply the coordination of unilateral behavior, and when actual collaboration in joint enterprises; when arrangements will deal only with the exchange of information, and when with joint and binding decision making.

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[21] The difficulties entailed by this particular limitation of the approach are encountered whenever the original formulations are applied to a geographical region other than Western Europe, or at the global level.

[22] A superb discussion of the origins and significance of the Westphalia conception is presented by Leo Gross, "The Peace of Westphalia, 1648–1948," in Richard A. Falk and Wolfram F. Hanrieder, eds., *International Law and Organization* (Philadelphia: J. B. Lippincott, 1968); an equally good discussion of the various modifications the Westphalia system has undergone is Falk's "The Interplay of Westphalia and Charter Conceptions of International Legal Order," in Richard A. Falk and Cyril Black, eds., *The Future of the International Legal Order,* Vol. I (Princeton: Princeton University Press, 1969).

[23] This is the analgous situation I wish to exploit for proposition and insights.

Further, in the context of the kinds of alternatives here posed, I view states as knowing what it is they will, and of being able to order the various things they will in terms of priority. Moreover, I view them as preferring to accomplish more of an objective rather than less, and of wishing to do so with the least necessary expenditure of limited resources. Finally, in keeping with the basic structure of the interstate system, I view states as preferring to limit their dependence upon others to the least necessary level above that assumed for all states.

The analysis will begin with the least complex case, that of one state pursuing one objective; and with the least complex issue, that of what combination of international and national organization that state will choose in pursuing its one objective. A state may tend toward greater international organization of an activity, or international performance of a task or resolution of a problem (with "international" referring, for the time being, to any "extranational" form), to the extent to which it discovers the inadequacy (or lack) either of one or of both of two resources. These are: (1) physical capabilities, which may be inadequate simply because there are not enough of them, or which may be irrelevant because the extant definition of property rights places the source of the problem within the jurisdiction of another; and (2) knowledge of cause/effect relations underlying either problems or solutions. These two resources will be referred to as "capabilities" and "techniques," respectively.

Thus, the inadequacy (or lack) of either capabilities or techniques, or of both, may lead a state to seek to cooperate with others. At the same time, however, cooperating with and thereby becoming dependent upon others for either capabilities or techniques itself poses a problem for that state, by giving rise to "interdependence costs,"[24] reckoned in such terms as circumscribed options or general loss of autonomy.[25] These are incurred over and above the more direct payments, to whatever institutional arrangement the state has selected, for the cost of performing the task. Given our assumptions, even though such costs may not appear significant, or may not be calculable, for any one particular arrangement, over the long run a state is expected to seek to keep such interdependence costs to the least necessary level. Hence, in calculating a state's propensity to organize activities internationally, not only the direct gains and the direct costs of performing a task with others will be considered, but the overall interdependence costs too will be included.

Thus, it follows that *the propensity for international organization will be determined by the interplay between the need to become dependent upon others for the performance of specific tasks, and the general desire to keep such dependence to the minimum level necessary.* A point demonstrating that relationship can now be defined.[26]

The General Model.[27] For the time being, imagine a world consisting of: two completely identical states—identical preferences, capabilities, techniques—states *A* and *B*, where *B* simply refers to the composite of "all other states in the system"; two fixed resources (that is, the level of capabilities and techniques is limited) to be expended in the pursuit of a given task; state *A* being confronted with the choice of through which combination of organizational arrangements, national and international, to perform the given task; an autonomy of decisional contexts, so that decisions made in one context remain independent of decisions made in another; constant returns to scale (that is, changes in inputs

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[24] The concept of "interdependence costs" is used by James M. Buchanan and Gordon Tullock, in *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962), to refer to the sum of "external costs" (those incurred through the actions of others in the collective arrangement) and "decision-making costs" (the costs of participating in a collective arrangement). Here I use the concept to mean a more general loss of independence or loss of control over one's own activities, resulting from the accumulation of collective constraints.

[25] Cooper, in *The Economics of Interdependence,* describes such constraints in international economic affairs.

[26] By propensity is meant what a state will *try* to accomplish, or that which it wishes and technically can accomplish; it does not define the ultimate outcome since that, of course, depends upon others as well. This latter dimension is introduced into the analysis in a later section.

[27] The figures accompanying the verbal argument which follows do not constitute a formal deductive model in the strict usage of the term. Instead, they are intended as diagrammatic representations of the various assumptions and modifications which are introduced into the analysis, and are utilized so that the latter can be more effectively demonstrated.

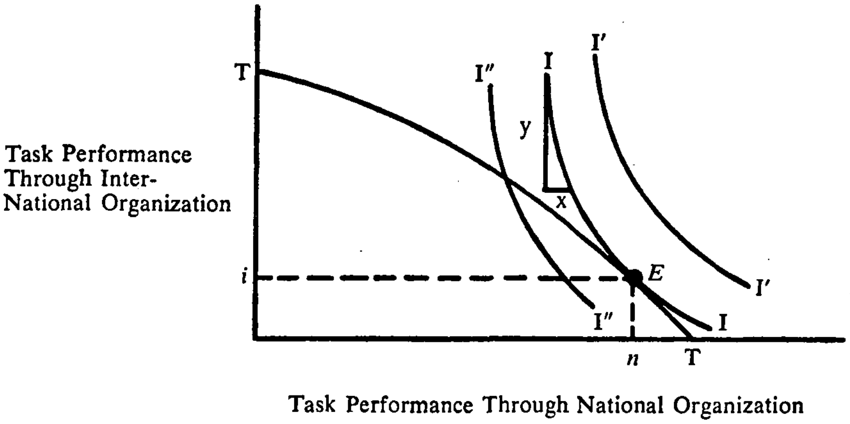

Figure 1. The General Model

by *A* into either international or national arrangements yielding proportional changes in the respective levels of task performance); and, finally, no direct interaction between *A* and *B*.

In calculating the combination of organizational arrangements *A* will select, the first problem is determining the optimum allocation of *A*'s resources (capabilities and techniques), between national and international organizational arrangements, given a fixed contribution to the latter on the part of *B*. Analytically, this determination consists of finding the locus of points at which the resources are so allocated that it is no longer possible to increase performance through international arrangements without decreasing the level of performance through national arrangements, or vice versa.[28] From such a locus of points, the feasible and efficient combinations of national and international task performance that *A* can secure can be plotted. Every point on the curve—called a transformation curve, and labeled *TT* in Figure 1—is both feasible and efficient; any point between the curve and the origin is inefficient (since greater *total* performance can be obtained), while any point beyond the curve, away from the origin, is infeasible (since it is beyond the scope of *A*). The slope of the curve at any point indicates precisely how much more international performance can be obtained by transferring resources from national arrangements, and vice versa.

Curve *TT*, however, plots only one of the two factors which go to make up our basic premise, the level of national resources. Juxtaposed to the "utility" (satisfaction) derived from cooperating with others in the performance of any one particular task, it was argued above, is a general desire to keep dependence upon others limited. These preferences are represented in the curves labeled *II* in Figure 1. They are "indifference" curves, plotting the combinations *A* views as being equally acceptable. That *A* derives satisfaction from the international performance of a task is represented by the very existence of indifference curves. That any state wishes to keep dependence upon others limited is represented by the slope of the curves: as shown on *II*, the ratio of *y* to *x* is quite large, indicating that before *A* would be willing to relinquish one unit of *x*, the payment in terms of units of *y* would need to be high indeed. An entire map of such curves exists, each one ranking preferences for a total level of utility.[29]

In sum, the curve *TT*, or transformation curve, plots what *A* is able to do: the curves *II*, or indifference curves, plot what *A* would like to do. What *A* will seek to do is defined at the point at which the two come together. Such a point, measured by the ratio of *i* to *n*, is point *E* in Figure 1. Here *A* is in equilibrium. For, at the point of tangency between the transformation curve and an indifference curve, their

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[28] Formally, this locus consists of the points of tangency between the "production isoquants" of the two goods. While there is no need to pursue it here, the interested reader might wish to consult Michael J. Brennan, *Theory of Economic Statics* (Englewood Cliffs: Prentice-Hall, 1965), Chap. 31; Edwin Mansfield, *Microeconomics: Theory and Applications* (New York; W. W. Norton, 1970), Chap. 14; and, in particular, Francis M. Bator, "The Simple Analytics of Welfare Maximization," *American Economic Review*, 47 (March, 1957), 22–59.

[29] The fact that the indifference curves are represented as being asymptotic to the axes indicates that diminishing marginal rates of substitution too are assumed; that is, extreme cases of complete internationalization and complete isolation are both excluded from the preferences of states in this analysis.

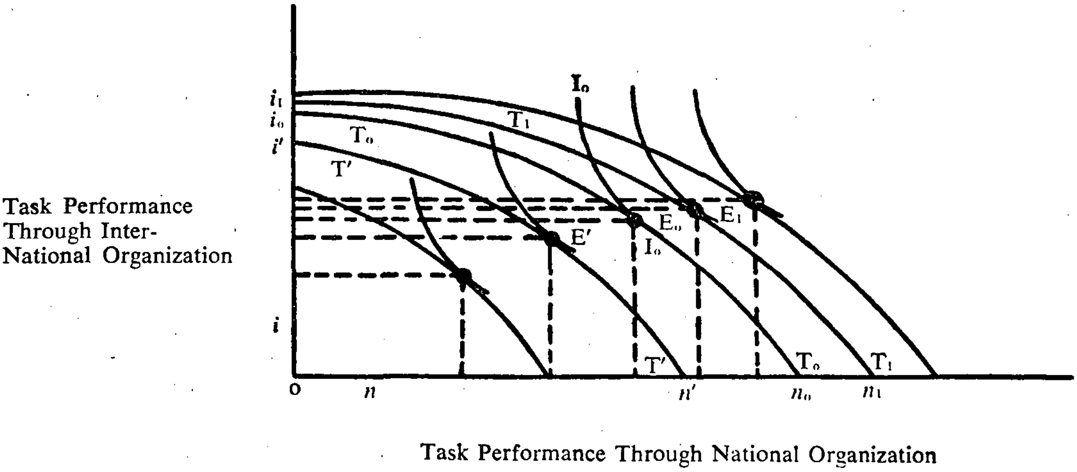

Figure 2. The Impact of Inequality of National Resources

slopes being equal, the extra benefit or satisfaction gained by foregoing one extra unit of *n* for one extra unit of *i,* or vice versa, is just equal to the extra cost of transforming one more unit of one into the other. At *E*

$$(1) \quad \text{MRS} - \text{MRT} = 0,$$

where MRS refers to the marginal rate of substitution in consumption (loosely speaking, the extra satisfaction gained), and MRT refers to the marginal rate of transformation in production (roughly speaking, the extra cost, of one in terms of the other).[30] The coordinates of the point *E* mark the combination of organizational arrangements *A* will select. Given our assumptions, a state can do no better, nor ought it do worse, than being at *E.*

Thus, the propensity for international organization, a resultant of the interplay between the need to become dependent upon others for the performance of particular tasks, and the general preference to keep such dependencies limited, is defined by the coordinates of the point *E.* While it may well be impossible to operationalize such a point in an empirical setting, there is no pressing need to do so here. For the purposes set forth, it will be sufficiently instructive to relax a number of the initial assumptions, to introduce additional conditions, and then to compare the different points of equilibria of nonidentical actors under dissimilar conditions.[31] The first assumption to be relaxed is the identity of states: inequality of national resources is introduced, and its impact postulated.

Inequality of National Resources. Since complete identity of states was assumed in the above formulation, only one point of equilibrium was determined. We shall now want to relate inequality of national resources to the propensity for international organization.

Figure 2 repeats the general model for state *A:* a transformation curve, T_0T_0, a set of indifference curves, *II,* and the point of equilibrium at point E_0. The transformation curve, it will be remembered, is the locus of points indicating the efficient and feasible combinations of both national and international task performance, and it assumes a fixed level of resources. If *A*'s level of resources were, however, actually lower than that assumed in the plotting of T_0T_0, clearly the overall level of possibilities would de-

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[30] The precise formulation would require taking partial derivatives of the transformation and utility functions, the point of equilibrium being where the slope of one minus the slope of the other equals zero. Where lower-case *u*'s and *t*'s refer to these partial derivatives, *a* to state *A*, the case for *B* being identical,

$$(1.1) \qquad \frac{u^a I_a}{u^a N_a} - \frac{t^a I_a}{t_a N_a} = 0.$$

For a geometric and algebraic demonstration, see James M. Buchanan, *The Demand and Supply of Public Goods* (Chicago: Rand McNally, 1968), chap. 2. I have found Buchanan's volume to be an enormously helpful formulation of public goods phenomena.

[31] In the context of more sophisticated and discrete analyses, it may, however, be desirable to operationalize the point *E*. In that case, some of the technical suppositions of the formulation, resting, as it does, upon differential calculus, might prove to be a constraint. In particular, it might be found that the resources available to a state are not perfectly substitutable, and that the objects of choice are not continuously divisible. While different mathematical systems (such as set theory) are now beginning to be utilized in this area of inquiry, much of the work in the theory of collective goods is based upon the mathematics of a prior vintage. For our exploratory intent, the latter will suffice.

crease, and the transformation curve would shift inward, toward the origin. Similarly, were A's level of resources to be higher, the transformation curve would shift outward, away from the origin. The original x-intersect (marking the total level of national performance A could secure if all of its resources were spent nationally) was at point n_0. In the case of lower resources the x-intersect will have shifted to a point such as n', and in the case of higher resources to n_1.

What of the y-intersect? Even though A's contribution within an international arrangement might decrease, it is reasonable to assume that the total level of international performance from which A is able to benefit does not decrease by a like amount, *provided* that the contribution of B remains constant. Hence, the y-intersect too will shift toward the origin, but *not* by as great a magnitude as the x-intersect. Similarly, in the case of higher national resources, the y-intersect will increase but *not* by as great a magnitude as the x-intersect, since the increased benefits from international performance will have to be shared with B. In short, rather than assuming constant returns to scale in the international performance of a task, we recognize and assume the more realistic situation of increasing returns to scale for countries with smaller resource bases, and the possibility of decreasing returns to scale for countries with high levels of national resources.

As a demonstration, five situations are plotted in Figure 2, two with higher, and two with lower levels of resources than the original (T_0T_0). Both n and i can be seen to increase as resources increase, and decrease as resources decrease, but not by like amounts. The points of equilibria, or the combination of the two which will be selected, however, indicate that the *ratio* of i/n *increases* the *lower* the level of national resources, and *decreases* the *higher* the level of national resources. More precisely, *there is an inverse relationship between the ratio of i/n and the total level of national resources.*

An additional implication is that a state's propensity to organize internationally will change over time, as its national resource base changes. This implication will now be explored, introducing another dimension, time.

Changes in Resources Over Time. If we take Figure 2 as representing the same state at different points in time, the postulated impact of changing levels of national resources over time can be demonstrated. In Figure 2, as national resources rise (represented by the transformation curves moving further away from the origin), the combination of i and n change: i increases, but at ever-smaller increments, and eventually levels off; n, on the other hand, continues to rise (as long as resources rise, and as long as the specified task remains to be performed). What this indicates is that—under the assumed conditions—*as national capabilities increase and become sufficient to perform a given task, the propensity for international organization* (in that instance) *decreases.*

Put differently, over the long run i will respond little, and eventually not at all, to increases in national resources, whereas n always responds positively. Were we to calculate a coefficient of responsiveness of i to changes in national resources, R, taking the percentage change in i over the percentage change in R, then

$$\frac{\frac{\Delta i}{i}}{\frac{\Delta R}{R}} \approx 0. \tag{2}$$

Likewise, the coefficient of responsiveness of national performance to increased resources will be positive.

Hence, *built into the international performance of any given task is a process of encapsulation*—ending in the situation in which no further commitments are made, and in which no further increase in the scope of the collective arrangement nor in its institutional capacity occurs. And the generally expected processes of task-expansion and spillover will *not* take place *unless* those factors now held constant change. Thus, to predict the growth and impact of collective arrangements internationally, it will not do—in view of the perspective here developed—simply to point to new problems that states will face (such as those generated by science and technology), then to posit new tasks for international arrangements, and finally to draw inferences on that basis. For unless the constants are shown to change, such growth will be truncated.

In the analysis thus far the cases selected have been simple and the conceptual categories utilized have been nondiscriminating. Nevertheless, a number of propositions, characterizing basic behavioral tendencies of states, have been developed:

(1) The propensity for international organization is determined by the interplay between the need to become dependent upon others for the performance of specific tasks, and the general desire to keep such dependence to the minimum level necessary.

The point of equilibrium was defined; the impact of varying conditions upon this point demonstrated that

(2) There exists an inverse relationship between the ratio of *i/n* and the total level of national resources.
(3) Over time, as national capabilities increase and become sufficient to perform a given task, the propensity for international organization (in that instance) decreases.
(4) Built into the international performance of any given task is a process of encapsulation, which will end in the situation of no further commitments to, and no further increase in scope or capacity of, the collective arrangements.

These propositions are based upon, and follow from, the assumed conditions.

To speculate intelligently about the probable impact of specific scientific and technological developments upon the processes of international organization, more refined categories and more complex cases will have to be introduced. But since these will be based on the analysis thus far, the above propositions must first be illustrated and the utility of this mode of analysis thereby demonstrated.

The Propositions Illustrated

Two fundamental changes, a preoccupation with the production of publicly-relevant goods, and a preoccupation with goods which are produced and/or purchased communally, are said to characterize the transformation of the advanced industrial societies. Our concern is with a parallel phenomenon internationally: the extent to which joint production and joint regulation of goods and services are selected by states, particularly in response to developments in science and technology. In offering some preliminary illustrative observations, I will limit myself, then, to the advanced industrial states, to those generating and most immediately affected by sciences and technologies.[32]

The General Model. The extent of international organization among the industrialized states is, first and foremost, the result of the interplay between the need to become dependent upon others for the performance of particular tasks, and the general desire to keep such dependencies to the minimum level necessary. The most telling illustration of this principle is offered by the most extensive experiment in international organization extant, the European Community. Pointing to the "very special conditions" out of which the Community grew, two of its students have concluded that it emerged

> . . . in part at least, as a result of the inability of national systems to process certain kinds of economic, social, and welfare demands. Its continued growth will be a function of its ability to provide decision capabilities that national governments acting alone cannot command.[33]

The concomitant desire to limit the dependencies incurred through collaborating with others has been exhibited by the European case as well, and has been most succinctly expressed by General de Gaulle. Speaking of collaboration in science and technology, he said

> . . . when it is opportune in a selected branch to join our inventions and money and skill with those of another country, we must choose the country nearest to us and whose weight would not crush us.[34]

Finally, the Community experience offers examples of the interplay between these two factors as well. Summarizing these tendencies on the part of member governments,

> . . . the history of *Euratom* bears out their fear of supranational European organizations in which decisions are taken by majority vote and individual countries cannot exercise a veto.[35]

The evidence suggests that one reason Euratom's performance has been so disappointing is that the final organizational arrangement turned out to entail dependencies far in excess of the need to become dependent—Suez was reopened, new sources of natural gas were discovered, new and larger tankers were introduced and ocean freight rates, therefore, were lowered; and the need for nuclear fuel consequently diminished. A less demanding organizational context, while equaling Euratom's successes, might have avoided its severe difficulties.[36]

Thus, national actors within the European Community appear to organize activities collectively, first and foremost, to meet certain salient demands which otherwise cannot be met. Once collective organization and performance are decided upon, their extent would appear to be limited by the desire to be no more dependent upon others than is necessary. This interplay is expressed by the point of equilibrium of the general model (Figure 1). To illustrate the postulated changes in that point of equilibrium, we will examine the behavior of the industrialized countries in two areas: (1) the development of their own scientific and technological capabilities, and (2) their response to the needs of the Third World. We would expect actual behavioral tendencies in both areas roughly to approximate those postulated; the illustrations which follow suggest that this is generally so. Yet systematic differences of behavior in the two areas appear to exist as well, and these suggest the direction that more discrete and discriminating analyses might take.

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[32] Even though the model deals with any form of international organization, the following illustrations are drawn from intergovernmental organizations, simply because data from these are more readily available.

[33] Leon N. Lindberg and Stuart A. Scheingold, *Europe's Would-Be Polity* (Englewood Cliffs. Prentice-Hall, 1970), p. 95.

[34] Cited in Gilpin, *France in the Age of the Scientific State,* p. 377.

[35] Gilpin, p. 407.

[36] I base this judgment on Gilpin's account, in the volume previously cited; upon an excellent analytical study of Euratom by Lawrence Scheinman ("Euratom: Nuclear Integration in Europe," in *International Regionalism,* ed. J. S. Nye, Jr., [Boston: Little, Brown, 1968]); and a detailed documentary and historical account by Warren B. Walsh (*Science and International Public Affairs* [Syracuse: The Maxwell International Relations Program, 1967]).

Inequality of National Resources. Under the conditions assumed, it was argued that an inverse relationship exists between the ratio of i/n and the total level of national resources. To illustrate this relationship, we shall first take the economic and social development of the Third World as a "task," and the members of the Development Assistance Committee (all except Portugal) as the universe of industrialized countries engaged in the performance of that task. The proportion of net official resources distributed to developing countries through multilateral agencies will constitute our measure of i, while that proportion distributed bilaterally will constitute our measure of n. We expect an inverse relationship between the multilateral/bilateral assistance ratio, and the total level of national resources, the measure of which is GNP. Table 1 displays this relationship.

The ratio of multilateral/bilateral assistance is indeed negatively correlated with the total level of national resources (GNP). The negative correlation is high, and almost equally so for the entire nine year period for which it was calculated. Hence there does appear to be an inverse relationship between i/n and the total level of national resources.

In addition, however, in examining Table 1 we find another and equally interesting inverse relationship, i.e., between the *percentage* of GNP devoted to development assistance, and the ratio of multilateral/bilateral assistance. This relationship suggests an additional proposition, namely, that the higher the *proportion* of national resources devoted to a particular task, the lower the ratio of i/n. This proposition is illustrated in the context of the funding of research and development as well.

Taking as a "task" the financing of research and development by those industrialized countries about which comparable information is available, we include two measures of i and two of n. For the former, the calculations were based upon total governmental expenditures for R & D abroad, and total governmental R & D expenditures in selected intergovernmental organizations. For the latter, the corresponding calculations were based upon total governmental expenditures for R & D, and total governmental domestic expenditures on R & D.[37] Again we expect an inverse relationship between the i/n ratio and the proportion of national resources (% GNP) devoted to R & D (GERD). Table 2 displays this relationship.

All four expressions of the ratio of i/n are negatively correlated with the proportion of national resources devoted to R & D. While calculations could not be performed for a period of years, it was possible to compare the "global" sample of industrialized countries with a smaller "European" sample. In all cases the inverse relationship is substantial, although it is systematically lower than in the case of development assistance distribution.[38] In other words, generally similar behavioral tendencies are exhibited in both areas, but differences too appear to exist; their significance is turned to later in the analysis.

Thus, as expected, the condition of possessing a certain level of national resources seems to be related to a state's propensity to organize the performance of a task internationally. In addition, however, the proportion of national resources a state has chosen to allocate to the performance of a task will show much the same effect. Both have interesting implications for the future, should national resources with which to perform tasks, and national priorities

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[37] Data on international R & D expenditures are difficult to obtain. The various sources utilized, and the various procedures by means of which the measures were arrived at, are described in the notes to Table 2.

While expenditures have changed since 1963/64, the *pattern* of expenditures appears reasonably stable. International R & D funding increased from 1963 to about 1968 (Louis Villecourt, "Forms of Cooperation," *Problems of Science Policy* [Paris: OECD, 1968]). Since then, "as far as the increasing share of national science budgets going to international projects is concerned in different OECD states, it seems that this share has either stabilized or even slightly decreased" (Louis Villecourt, private communication to the author, 4 June 1970).

[38] This is true of the relationship between the i/n ratio and GNP as well, the R & D correlations there being lower still.

Table 1. Development Assistance Correlations:[a] Showing Inverse Relationship Between National Resources and the Ratio of Multilateral/Bilateral Assistance 1960–1968

	(1)	(2)	(3)	(4)
1960[b]				
(1) GNP	—			
(2) Total assistance	.76	—		
(3) Total as % of GNP	.37	.71	—	
(4) Ratio of multi/bilateral	−.43	−.81***	−.85***	—
1962[c]				
(1) GNP	—			
(2) Total assistance	.92	—		
(3) Total as % of GNP	.71	.90	—	
(4) Ratio of multi/bilateral	−.76**	−.77**	−.68*	—
1964[d]				
(1) GNP	—			
(2) Total assistance	.93	—		
(3) Total as % of GNP	.69	.87	—	
(4) Ratio of multi/bilateral	−.73**	−.78***	−.70**	—
1966				
(1) GNP	—			
(2) Total assistance	.95	—		
(3) Total as % of GNP	.57	.75	—	
(4) Ratio of multi/bilateral	−.68**	−.79***	−.75**	—
1968[e]				
(1) GNP	—			
(2) Total assistance	.95	—		
(3) Total as % of GNP	.37	.56	—	
(4) Ratio of mutli/bilateral	−.83***	−.81***	−.31	—

$P <$ *** .001
** .01
* .05

[a] An examination of scattergrams, on which actual amounts were plotted, evidenced the effect of one or two extreme values, at both ends, in GNP and in Total Assistance. Therefore, the correlations here reported are based on rankings, using the *rho* coefficient. Correlations were computed for each year, from 1960–1968, and those reported here are representative. Moreover, the correlations were also calculated on the basis of actual amounts, with the coefficient being systematically lower, but still substantial and negative.

[b] The sample is drawn from members of the Development Assistance Committee; for 1960 these included Belgium, Canada, France, Germany, Italy, Japan, Netherlands, United Kingdom, and the United States.

[c] In 1962, disbursements of Austria, Denmark, and Norway were reported as well.

[d] In 1964 Sweden and Australia too were added.

[e] The 1968 DAC group included Switzerland.

Sources: For GNP: OECD, *National Accounts of OECD Countries: 1957–1966* (Paris: 1968); OECD, "The OECD Member Countries," 4th Year, 5th Year, 6th Year, Supplements to the *OECD Observer;* United Nations, *Yearbook of National Accounts Statistics*, Vol. I (New York: 1969). For development assistance: OECD, *Development Assistance Efforts & Policies*, annual reviews, 1961–1969.

about what tasks to commit resources to, change.

Changes in Resources Over Time. In the absence of further specifications, it is difficult to illustrate the proposition that as national capabilities become sufficient to perform a given task, the propensity for international organization in that instance decreases. One would need to calculate, for instance, precisely when capabilities had become adequate. For illustrative purposes only, we might consider the case of three small Euro-

Table 2. R & D Correlations:[a] Showing Inverse Relationship Between Proportion of National Resources Devoted to R & D, and the Ratio of International/National R & D Expenditures 1963/1964

	"Global" Sample[b] (1)	European Sample[c] (1)
(1) Percentage of GNP Devoted to Gross National Expenditures on R & D (GERD)	—	—
(2) Governmental R & D Expenditures Abroad/[d]Total Governmental R & D Expenditures[e]	−.53	−.54
(3) Governmental R & D Expenditures in Selected Intergovernmental Organizations/[f]Total Governmental R & D Expenditures	−.58*	−.63*
(4) Governmental R & D Expenditures Abroad/Governmental Domestic R & D Expenditures[g]	−.52	−.53
(5) Governmental R & D Expenditures in Selected Intergovernmental Organizations/Governmental Domestic R & D Expenditures	−.56*	−.61*

$P<$*.05

[a] The various ratios, on the basis of which these correlations were computed, are far more comparable than are the actual dollar amounts in the case of the development assistance correlations. There was, therefore, no reason not to use the actual ratios in computing *r*. Nevertheless, because of the small sample, and to assure maximum possible reliability, a number of additional calculations were performed, including computing levels of significance (with the results as shown in the body of the table), and generating scatterplots for each set of relationships (with visual confirmation of *r*).

Moreover, since no single source of data on international R & D expenditures is either complete, or completely reliable, every attempt was made to assure the *comparability* of the figures collected from more than one source. In addition, where discrepant figures could not be avoided, the various ratios were calculated on the basis of both. In *no* case did using one as opposed to another figure affect the relative positions of the countries in the sample, nor was the final coefficient affected in any significant way.

[b] The "global" sample of highly industrialized countries for which comparable R & D expenditure statistics could be found were: Austria, Belgium, Canada, France, Germany, Italy, Japan, Netherlands, Norway, Sweden, United Kingdom, United States.

[c] Since the "selected intergovernmental organizations" might be thought to be biased in favor of the European countries, an additional set of calculations on these alone was performed. The European sample consists of all countries listed in note *b* above, but Canada, Japan, and the U.S.

[d] The basic figure for governmental expenditures abroad came from OECD's *ISY* (see *Sources*, below), which included an account labeled "Governmental extramural expenditures for R and D to abroad." These were admittedly incomplete and incorrect in some cases, apparently so in others. Where possible, the OECD country reviews of science policy (see *Sources*, below) were consulted and, where necessary, adjustments were made.

[e] The basic figure for total governmental expenditures came from *ISY*, adding expenditures to abroad to total intramural expenditures (i.e., domestic). Since the same conditions held as described in note *c* above, the identical procedures were followed.

[f] Since, to my knowledge, no published source on national participation in internationally collaborative R & D ventures at *all* levels, including the regional and global, exists, a set of intergovernmental organizations, for which comparable data could be found, was selected. Thus, expenditures in "selected intergovernmental organizations" refers to: UNESCO, IAEA, WMO, the Science Committee of NATO, ENEA, CERN, EURATOM, ELDO-ESRO. These figures derive from national responses to an OECD questionnaire; I am grateful indeed to M. Jean-Jacques Salomon, Head of the Science Policy Division, for making these available to me.

[g] The basic figure for total governmental domestic expenditures came from *ISY*. Since the same conditions held as described in note *c* above, the identical procedures were followed.

Sources: For GNP (at market prices): OECD, *National Accounts of OECD Countries: 1957–1966* (Paris: 1968); for GERD: OECD, *International Statistical Year for Research and Development*, Vol. II, *Statistical Tables and Notes* (Paris: 1968).

The remaining statistics, with the exception of expenditures in selected intergovernmental organizations, are from *ISY*, and from the following country reviews: OECD, *Reviews of National Policy for Science and Education: Sweden* (1964); OECD, *Reviews of National Science Policy: Belgium* (1966); *France* (1966); *United Kingdom-Germany* (1967); *Japan* (1967) *United States* (1968); *Italy* (1969); *Canada* (1969).

The figures for international organizations came from: OECD, *National Participation in International Scientific Undertakings: Replies received from Member Countries*, Interim Committee Reference Papers on "Problems of International Co-operation in Scientific and Technological Research," prepared for the Ministerial Meeting on Science, 12th and 13th January, 1966.

Table 3. Increasing National Resources and the Ratio of Multilateral/Bilateral Assistance

	1962		1968	
	GNP†	Ratio	GNP	Ratio
Austria	7.29	14.167	11.40	.951
Denmark	7.44	9.571	12.39	.816
Norway	5.32	4.750	9.02	.840
U.S.*	569.12	.079	880.77	.075

Sources: Same as for Table 1.
† Billion $U.S.
* Included for comparative purposes.

pean states which first appeared in the Development Assistance Committee in 1962—Austria, Denmark, and Norway—and compare the ratios of i/n in that year with the much reduced ratios in 1968. These are displayed in Table 3. GNP, of course, increased year by year.[39]

Similarly, it is difficult to illustrate the proposition that built into the international performance of any given task—all else remaining constant—is a process of encapsulation which will ultimately result in the situation of no further commitments to, and no further increase in, the scope of the collective arrangement. Here, for instance, we would need to specify just how long the long run is. But again, for purely illustrative purposes, consider the "coefficients of responsiveness" of equation (2) as applied to multilaterally distributed aid (c_i) and bilaterally distributed aid (c_n), respectively, where GNP is taken to represent the level of national resources (R). Taken over the entire range of years, 1960–1968, $c_i = .001$, or very nearly zero, even though the calculation is based upon the nine members of the DAC in 1960, whereas the 1968 calculation is based upon fifteen members. On the other hand, taken over the entire range of years, $c_n = .114$.

My intent is to develop a theoretical system on the basis of which to analyze the impact of scientific and technological developments upon international organization. A number of basic behavioral tendencies of states, under varying conditions, have been postulated, and these have been illustrated in the context of some actual issue areas. The analysis and illustrations suggest that as levels of national resources become inadequate for the performance of tasks —as is generally argued to be the case with problems and possibilities posed by scientific and technological developments—the propensity to organize the performance of such tasks internationally will increase. But this initial tendency may be counteracted under several conditions: first, should national resources in general increase; second, should the proportion of national resources devoted to the performance of a task increase, as a result, for example, of a reordering of national priorities.

In addition, the illustrations suggest a further dimension which has been neglected thus far, that states' general propensities may come to be modified depending upon the extent to which it is situationally possible to act these out. For example, the inverse relationships reported in Table 1 are systematically more substantial than those in Table 2. One possible explanation is that the basic behavioral tendencies the model portrays operate to a greater extent in the area of development assistance funding than in the financing of research and development. This in turn may be due to the greater unilateral control the industrialized states have over aid and its distribution than over the more intense and immediate interdependencies generated by science and technology. To pursue these possibilities, and to engage in more specific speculation, at least one crucial dimension must be added to the model: the impact of the behavior of one state upon another.

Theoretical Extensions and the Impact of Selected Technological Developments

Our analysis of the conditions under which a state would seek the joint production or joint regulation of goods and services internationally, and of the conditions and policy decisions which might counteract such a tendency, explored only an exceedingly restrictive case: that of no direct interaction between states; that of a completely undifferentiated conception of task or activity; and that in which the international organizational arrangement was simply any extranational form. Yet we know that the activities of one state are affected by others, and, in turn, have consequences for others—that there is a collective dimension to the behavior of states, which may well be increasing in scope and in importance as a result of precisely the kinds of issues we wish to analyze. In addition, we know that different kinds of activities lead to different forms of organizational arrangements internationally. Here I will seek to demonstrate that different kinds of activities will lead to different organizational forms, *not* because of *a priori* substantive differences, but because of the impact of the collective dimension that the behavior of states may exhibit.

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[39] The percentage of GNP devoted to development assistance in 1968 by each of the three was at least twice that of 1960. Comparable R & D information was not available.

Hence, a number of extensions in the analysis will be developed and illustrated. The first postulates some modifications in a state's general propensity to organize activities internationally (as defined by point *E* in Figure 2), resulting from the recognition that its behavior in the performance of a task is affected by and has consequences for others. The second differentiates various kinds of international collaborative arrangements, which may subsequently be created, from international organization in general. Finally, these theoretical extensions will be illustrated in the context of internationally significant issue areas which are affected by scientific and technological developments.

The behavior of a state in the performance of a task may exhibit collective characteristics. To the extent that these affect others significantly, we might expect some measure of collective activity to follow. In exploring this relationship, we must first explicate more rigorously the notion of "collective characteristics."

According to the classical definitions, goods and services are of two polar types: either purely private or purely collective (or public). Purely private goods and services ($X_1, X_2, \ldots, X_n$) can be parceled out among different individuals ($1, 2, \ldots, s$) so that this relation is satisfied:

$$(3) \qquad X_j = \sum_{i=1}^{s} X^i_j \, .$$

That is, the good or service, j, is completely divisible, so that the total quantity of it available to the group equals the sum of the quantities of it available to the individuals within the group. On the other hand, purely collective goods and services ($X_{n+1}, X_{n+2}, \ldots, X_{n+m}$) are common to the group, in the sense that the benefits are perfectly indivisible among the separate individuals, and this relation is satisfied:

$$(4) \qquad X_{n+j} = X^i_{n+j} \, .$$

That is, the total quantity of $n + j$ available to the group is precisely the same as that available to any i^{th} member of the group, and no one individual's consumption of the good in any way subtracts from the consumption of any other.[40]

Thus, for example, were *A*'s performance of a task to exhibit pure collective characteristics, the benefits of its activities would be extended to each and every member of the entire system. If those benefiting from *A*'s activities fail to pay for those benefits, and if state *A* acts in accordance with our assumptions, then *A* might cease those activities providing collective benefits, or seek to have their production organized collectively.

These are polar types, and their applicability would appear to be limited to very special cases indeed. Yet, a closer examination of the concept of collective goods reveals two major dimensions, yielding a more discrete and more generally applicable fourfold classification.

The first basic dimension of a collective good is that, rather than being perfectly divisible, in the sense that each producer captures the full benefits of his own product, the good is in "joint supply," or is "indivisible." By joint supply is meant that once the good or service is produced or performed, for and by one producer, its extension to others is facilitated: "once produced, any given unit of the good can be made equally available to all." And, up to a point, its extension to one additional individual does not imply a corresponding reduction in the quantity of that good available to others.[41]

Simply because such an "indivisibility of product," or "equal potential availability" exists, however, does not necessarily imply that the good *must* be made equally available to all; it may be perfectly possible to exclude outsiders. In essence, it means only that the opportunity cost of extending the good or service to one additional individual may be virtually negligible.[42]

There is, in addition, a second basic dimension of a collective good. Instead of being able to limit the utilization of it to those participating in its production, a state may confront "impossibility of exclusion," or "nonappropriability of costs." In other words, it may not be possible to exclude others from sharing, or to charge them the full cost of sharing, the benefits of the good. (Or, it may not be possible to exclude oneself from the suffering caused by the production of a good by others, nor to obtain compensation for such suffering.)[43]

But impossibility of exclusion or nonappropriability of costs do not necessarily imply that the good in question is in joint supply; in the technical sense it may be perfectly divisible. What is implied is that there exist "imperfections in property titles," making it impossible to contain benefits or exclude suffering.[44]

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[40] These standard definitions of private and public (or collective) goods were first developed formally by Paul Samuelson, in "The Pure Theory of Public Expenditures," *Review of Economics and Statistics*, 36 (1945), 387–389.

[41] The classification and the various definitions are based upon an extraordinarily clear presentation of the concept of collective goods by J. G. Head; see "Public Goods and Public Policy," *Public Finance*, 17 (1962), 197–219; this particular definition appears on pp. 201–202.

[42] Head, p. 207.

[43] Head, p. 203.

In sum, the two basic dimensions of a collective good lead to the following four-fold classification of all goods and services:

	Perfectly Divisible	In Joint Supply
Possibility of Exclusion/ Appropriability of Cost	(1)	(2)
Impossibility of Exclusion/ Nonappropriability of Cost	(3)	(4)

Each of these four sets of characteristics that national behavior may exhibit will be briefly discussed in turn, and I will show how each might lead to a different form of internationally collaborative arrangement. The only additions to the model of the previous sections, then, are the collective dimensions of state behavior—all else remains constant.

(1) Divisibility and Appropriability. In the strict sense here employed, *A*'s production of a good or service which is perfectly divisible and from which others can be kept from benefiting (or be charged for benefiting), exhibits no collective dimension whatever. Yet, it is the production of just these goods and services which accounts for most of the activities of intergovernmental organizations. This is the case in part for the kinds of reasons explicated in the previous sections. In addition, all states may bring to bear various efficiency criteria, such as economies of scale, and therefore seek the collaborative production of a particular good or service. Thus, simply in order to be able to do what it cannot now do, or to do more, or more efficiently what it is already doing, a state may enter into international arrangements facilitating such desires.

What kinds of organizational arrangements would[45] these be? Their purpose, clearly, would be to facilitate or enhance a particular national capacity—to enlarge the range of what is technically possible for each member, in the performance of a particular task.[46] Their role would essentially be limited to the coordination of national activities. For given the grounds for initially entering into such an arrangement, the interdependence costs of a more demanding organizational arrangement would soon exceed the benefits obtained from it.[47] An illustration follows.

International Collaboration in R & D. The most common example of international collaboration in the areas of science and technology is the joint production of essentially private goods: national scientific and technological capabilities. The European case offers a number of illustrations. A European technological community, *The Guardian* has argued,

> . . . is the real Europe of the future. To build it will be a test of nationalism versus rationalism. . . . [T]he only alternative is economic decline, and probably complete domination by the United States.[48]

The European countries have long complained of an Atlantic "technology gap" in terms of a brain drain, the production of scientific papers, innovation, the technological balance of payments, and so forth.[49] And, to derive the benefits of economies of scale, more integrated European scientific and technological efforts have often been proposed and sometimes executed.[50]

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[44] Although the two properties appear to be similar, joint supply and problems of exclusion "are conceptually quite distinct properties . . . and are in no way related. A clear differentiation between them is therefore extremely important to preserve." (Head, p. 210)

[45] I emphasize *would,* because the idealized conditions of the model are assumed to hold. According to the model, equilibrium for the entire system (here taken to mean the establishing of an organization) would be reached when this relation is satisfied:

$$MRS^a + MRS^b = MRT. \quad (1.2)$$

In other words, the *summed* marginal rates of substitution (summed over all relevant states) must be equal to the marginal rate of transformation.

[46] In the terms of Figure 2, the transformation curve for each state would shift outward, away from the origin, or at least no state would be left worse off as a result of some being better off.

[47] The example of Euratom, as described above, and in particular the rather fundamental changes in the organization of joint research activities effected between 1968 and the present, appear to attest to this conclusion.

[48] Cited by J.-J. Servan-Schreiber, in *The American Challenge* (New York: Avon Books, 1969), p. 112.

[49] The technology gap, and various European responses to it, are discussed in Gilpin, *France in the Age of the Scientific State, passim.* OECD has conducted a major study of the gap, and its findings have been published in a number of volumes. See, most importantly, *Gaps in Technology: Analytical Report* (Paris: OECD, 1970).

The Americans, in turn, have recently begun to complain that the technology gap is shifting in their disfavor, citing an alleged increasingly unfavorable technological balance of payments. These allegations are examined in Philip M. Boffey, "Technology and World Trade: Is There Cause for Alarm?" *Science,* 172 (2 April, 1971), 37–41.

[50] The most famous exhortation was that of Servan-Schreiber, in *The American Challenge.* A more sophisticated analysis and set of proposals, in much the same tradition, may be found in Christopher Layton,

But the basic reasoning is not limited to the Europeans: ". . . cooperation enables the United States to complete its national research efforts through the division of labour among many countries"[51] as well.

The organizational arrangements through which such collaboration is facilitated include standard intergovernmental organizations; in addition, still less demanding arrangements are being developed. Within ENEA, for example, specific member countries may participate project by project, without involving other OECD members not interested in a particular concern.

> This minimizes the extent of bargaining and has led to the development of co-operation based on common self-interest, which is perhaps the most sound basis for intergovernmental co-operation in science.[52]

In assessing how future scientific and technological developments might come to affect international organization, one arrives at the conclusion that in a great many cases states will respond to problems and possibilities such developments may pose by seeking the extension of national capacities through joint production facilities and information services. Because these arrangements are thought to introduce a collective dimension into the interstate system, we generally view them as potentially modifying that system. Yet, being producers of essentially private goods, which facilitate and enhance national capacities, their first consequence will be to augment the ability of states to act in that system.

(2) Joint Supply and Appropriability. The second case which may affect a state's basic propensity arises when the product of one of its activities is in joint supply, in the sense that extension of that good to others is facilitated, even though others can be excluded or charged for it. If other states were of the impression that *A* would supply the good or service in any case, they would have no incentive to contribute to its production. But even if others were willing to contribute, the opportunity cost of supplying the good or service to the last user might well be negligible.[53] Finding itself in a situation of this kind, state *A* has a number of available alternatives: exclude others, and ignore the social pressure which may result; extend the good to others and absorb the cost; cease the activity altogether; or seek to organize the production of the good internationally in the first place, with all contributing from the beginning. Any or all of these are possible, depending upon circumstances. The international production of such goods becomes more likely, however, to the extent that *A*'s situation is mutual among states and that the interdependence costs of so doing are acceptable.

What kinds of international arrangements would these be? Their purpose would no longer simply be to enhance or facilitate national capacities and behavior. In addition, they would be designed to compensate for the decentralized structure of the interstate decision-making system, by organizing internationally those national activities exhibiting joint supply. Their role, then, in addition to producing a particular good or service, would include at least the partial introduction of collective criteria into the interstate decision-making system. Some examples follow.

Observation, Surveillance, and Monitoring. Many cases of joint supply with possibilities of excluding others, in the areas of science and technology, concern the observation, surveillance, and monitoring of the earth's surface, the climate, and the oceans. In most cases, however, the full realization of these techniques and, therefore, their full significance for the objectives and capabilities of states, are still to come. Thus far, a great many examples have concerned primarily one state, the United States, producing a good or service, and then—sometimes through international auspices—choosing to extend it to others and to absorb much of the cost of so doing. Daily pictures of global weather systems, taken from U.S. satellities, and distributed through the World Weather Watch, are one example. Another example is the various experiments carried out under the auspices of the Global Atmospheric Research Program. But as these experiments

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European Advanced Technology: A Programme for Integration (London: George Allen & Unwin, 1969).

[51] OECD, *Reviews of National Science Policy: United States* (Paris: 1968), p. 307.

[52] Alexander King, "International Scientific Relations: Introduction," *Problems of Science Policy* (Paris: 1968), p. 141. King is Director, Directorate for Scientific Affairs, OECD.

[53] More precisely, were the conditions of our model to hold, *A* would seek to impose the *average* cost upon each user. However, as long as some states, who would be willing to pay the *marginal* cost, are excluded by the average cost, Pareto-efficient pricing does not exist. And, in a world of optimum conditions, such as that described by our model, "the position could be substantially improved by lowering the uniform price toward the true marginal social (opportunity) cost of supplying the service to the last user" (Head, "Public Goods and Public Policy," p. 212). If this were negligible, *A*'s compensation would amount to virtually nothing.

have become more costly, they have also become more collaborative.[54]

Much the same is true of monitoring systems and earth-sensor satellites. When they become operational, they will have an immensely diverse range of applicability, from prospecting to pollution control. As national policies come to depend upon the capabilities provided by these technologies, and as the costs of their production and operation and the costs of collecting and processing the data they will generate become serious, no state will simply extend these services to others. And those states excluded will pressure for inclusion. Thus, cost-sharing plans for both resource surveys and pollution monitoring, organized internationally from the beginning, are already being discussed and agreed upon in the UN and elsewhere.[55]

(3) Divisibility and Nonappropriability. The third case that may affect a state's basic propensity is that in which the good or service in question is strictly divisible, but because of "imperfections in property titles," i.e., the nature of political jurisdictions, others cannot be excluded from benefiting from it or cannot protect themselves from any disservice it might be causing them. If other states are enjoying the benefits of *A's* production of a good or service, and *A* can in no way exclude them or charge them the cost of partaking, it would be unrealistic—given our assumptions—to expect them to contribute voluntarily. Or, if other states are suffering from *A*'s production of a good or service, and cannot exclude themselves from such suffering, it would be unrealistic to expect *A* voluntarily to offer compensation. In both cases, a divergence between private and social costs results, as *A* would tend to underproduce the first kind of good, and to overproduce the second. (In neither case would the relation in [1.2] be satisfied.) Again a number of alternatives are available; but to the extent to which such divergences become costly, and mutual, the international production of the first kind of good and the international regulation of the second kind become more likely.

These would be still different organizational arrangements. In performing a specific task, they would be required to compensate for those "imperfections in property titles" within the interstate system which generated the divergence between private and collective costs in the first place. Within a task-specific context, their role would include introducing and representing definitions of collective ownership and jurisdiction.

Exploitation of the Seabed. The exploitation of the seabed beyond the limits of national jurisdiction has become a particularly interesting case of this third kind of good. It is all the more interesting because it indicates clearly how political factors interact with technological developments to define a situation.

Until recently, the exploitation of mineral resources and deposits was considered to be a purely private good and was enshrined as such in the Conventions of 1958.[56] These legally defined five zones: internal waters, the territorial sea, contiguous zones, the continental shelf, and the high seas. The delimitation between territorial and high seas was not universally fixed; and the definition of the continental shelf has raised still greater uncertainties.[57] Essentially, the latter has meant that a state can exploit the seabed off its coast for as far as its technology will take it. Both the technologies facilitating exploitation at ever greater depths, and subsequent exploitation, have continuously advanced.[58]

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[54] For a review of these various projects, see Nancy Gruchow, "Weather Services: Working toward Worldwide Forecasts," *Science,* 168 (17 April, 1970), 352–353. The administrative and decision-making arrangements of the GARP observational experiments are described in, WMO-ICSU, "Report of Interim Planning Group on GARP Tropical Experiment in the Atlantic, London, July 1970," *GARP Special Report No. 2* (Geneva: August 1970).

[55] In the context of the UN these issues are reviewed in "Issues Before the 25th Assembly," *International Conciliation,* 579 (September 1970), 59–64; see also the recommendations prepared for the 1972 Stockholm conference on the human environment (UN, A/CONF. 48/PC.9; 26 February 1971). The recent US initiative to establish a UN environment fund, which would include financing for monitoring, should be noted as well ("Nixon Offers Environment Plan," *International Herald Tribune,* February 9, 1972).

[56] The texts are reproduced in Norman J. Padelford, *Public Policy for the Seas* (Cambridge: The MIT Press, 1970).

[57] National sovereignty over the continental shelf and its subsoil was agreed to, but the area itself was barely defined:

> For the purposes of these articles the term "continental shelf" is used as referring (a) to the seabed and subsoil of the submarine areas adjacent to the coast but outside the area of the territorial sea, to a depth of 200 meters *or, beyond that limit,* to where the depth of the superjacent waters admits of the exploitation of the natural resources of the said area; (b) to the seabed and subsoil of similar areas adjacent to the *coasts of islands.*

Cited in Padelford, *Public Policy for the Seas,* p. 69; emphases added.

[58] For a review, a suggestion of what the consequences could be, and an expression of fear of what the consequences will be, see Wolfgang Friedman, *The Future of the Oceans* (New York: Braziller, 1971).

States not possessing the requisite technology, those which are landlocked, and developing states in general have feared the loss of the seabed to unilateral national claims. They, in turn, have sought to have the area beyond the limits of national jurisdiction declared "the common heritage of mankind," and to have an equitable distribution system established. Although the issue has been before the U.N. since 1967, differences between the developed and developing countries prevented substantive agreement for three years. In December 1970, a Declaration was adopted by the General Assembly setting forth fifteen principles, including the principles that the area *is* the common heritage of mankind and that exploration and exploitation should be governed by an equitable international regime, which is to be established.[59]

While the issue is far from settled, it is apparent that the nature of the seabed is becoming redefined; "imperfections in property titles" are being seriously discussed, and will be compensated for by establishing an international regime. How might this redefinition be coming about? Unilateral claims to the seabed can be made by states with the requisite technology; *all* coastal states can threaten such claims. And they can make these threats credible, irrespective of levels of technological development, by extending their territorial jurisdiction and/or by licensing corporations to exploit the seabed off their shores. No state can then avoid suffering the loss of the seabed through unilateral and competitive exploitation. That is, the situation has come to resemble our third case, and the proposed collective arrangement is designed not only to facilitate the performance of a particular task—the exploitation of the seabed—but also to compensate for imperfections in the basic definitions of political jurisdiction by establishing an international regime.

(4) Joint Supply and Nonappropriability. The fourth and final set of characteristics approximates a pure collective good; equal potential availability to all exists once the good or service is provided for one state; and it is impossible to exclude other states from sharing in the benefits (or for other states to protect themselves from the suffering) provided by the good or service. In other words, state *A* may be providing a good or service which *must* be extended to all; or *A* may find it *must* suffer from another's good or service, in the sense that it cannot protect itself. To the extent that such situations become mutual and costly, the international production and/or regulation of the goods in question becomes likely.

The kinds of international organizational arrangements these situations would lead to would be of a fourth variety. They would perform a particular task of production or regulation. But in so doing their purpose would be to compensate for both the decentralized nature of the interstate decision-making system *and* for the definition of political jurisdiction and ownership. Their task, in sum, would include the introduction of elements of collective decision making *and* collective ownership into a particular activity.

Large-Scale Climate Modification. An enormously important case of this fourth kind of good will emerge if and when technologies for weather control and for intentional climate modification become realized.[60] The case of weather control provides a localized example. Insofar as the amount of moisture in the atmosphere is fixed, rainmaking by one state will deprive others of a resource rightfully theirs—others *must* suffer from the behavior of one. Climate modification is a larger scale and still more pressing case of the same phenomenon—once produced by one, the "good" must be extended to all within the system.

Although experiments in rainmaking and localized weather control are quite common, the modification of the climate is some ways off. Given our set of assumptions, both kinds of modifications will be considered to be purely private goods until the suffering (actual or anticipated) becomes costly and mutual. At that time, we would expect both collective ownership and collective decision-making systems to be established to participate in the production and regulation of this particular activity.

In sum, the analysis of the preceding sections explored only a restrictive set of behavioral and organizational forms. These indicated the general conditions under which joint production and/or regulation would or would not be sought. In the present section, on the basis of more specific characteristics exhibited by the behavior of states, more specific organizational forms were explored and illustrated. The fourfold classification by no means exhausts the range of possibilities.[61] But, together with the basic model, it does facilitate the explication of some fundamental dynamics of international organization, their application to specific cases, and systematic speculation about future possibilities.

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[59] These principles are briefly summarized in the *UN Monthly Chronicle,* 8 (April 1971), 21–26.

[60] The state of technology of weather control is assessed in Allen L. Hammond, "Weather Modification: A Technology Coming of Age," *Science,* 172 (7 May, 1971), 548–549.

[61] For example, every activity could be analyzed from the perspective of the second-order consequences it

Conclusions

Two fundamental changes are said to characterize the transformation of the highly industrialized societies: a preoccupation with the production of publicly relevant goods and a preoccupation with goods which are produced and/or purchased communally. The concern of this paper has been a parallel phenomenon internationally, namely, the joint production and joint regulation of goods and services by the advanced industrial states. In particular, I have sought to explore the impact of scientific and technological developments upon this process, and to suggest some consequences of collective decision making and collective ownership for future international organization.

What is more, this inquiry sought explicitly to avoid evolutionary or functionalist assumptions. Instead, the processes of international organization were here formulated as being generated by states selecting from among alternate modes of organizing activities, both national and international, given certain constraints and possibilities, and given an assumed *modus operandi*. This analysis leads to a number of conclusions.

First, *logically*—in the dynamics of the internationalization of tasks as here defined—there is little that is unique about science and technology, little that inherently differentiates the issues they raise from other kinds of issues. The utilization of specific sciences and technologies (or of the category in general) as independent variables or as "issue-areas" in the analysis of international organization is not, then, the most propitious perspective. Such *a priori* substantive differences as might appear to exist among international trade, monetary affairs, pollution, and the exploration and exploitation of outer space and of the seabed do not determine the calculations of states. Instead, the decision-making calculus of states is determined by the configuration of problems and possibilities these activities pose for states *as organizational systems* having certain objectives and capabilities and acting in accordance with certain behavioral tendencies in the context of a particularly structured interstate system. As described in the previous section, by facilitating national behavior while extending its benefits due to joint supply, by offering new capabilities while raising problems of nonappropriability, and so forth, certain scientific and technological developments *are* interacting with states' and systems' attributes in such a way as to lead to more and to different kinds of collective activity.

Second, the central concern now pursued in the study of international organization is the evolution of collective actors—how organizations come to be established, how they come to grow, to have important functions, and to acquire authority "above" the level of states. The analysis here has been of a prior phenomenon —prior both logically and chronologically— namely, the *devolution* of existing structures: the complex modification or mutation of the modern state and modern interstate system, as a consequence of the introduction of collective dimensions into decision making and into definitions of political jurisdiction and ownership internationally. Not the emergence of "higher" forms of sociopolitical organization "above" states, then, but the differentiation and disaggregation of existing forms of organization, are suggested by this analysis as being the central phenomena in international organization today.[62]

Third, the devolution of existing structures appears to be an issue-specific and actor-specific process. It is asymmetrical, reflecting differences in national capacities to perform different tasks, as well as discontinuous, reflecting the differential impact of interdependence costs in different issue areas and for different states. Moreover, it appears to generate issue-specific and actor-specific collective arrangements, existing at different levels in the interstate system,

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generates. The vast literature on externalities could then be employed. (Among the finest works on externalities is William J. Baumol, *Welfare Economics and the Theory of the State* [Cambridge, Mass.: Harvard University Press, 1965]). Or, each activity may be classified according to whether it is perceived as being distributive, redistributive, regulative, or self-regulative, as defined by Theodore J. Lowi. (See, for example, "American Business, Public Policy, Case Studies, and Political Theory," *World Politics*, 16 [July 1964], 677–715; Robert H. Salisbury has extended Lowi's work in "The Analysis of Public Policy: A Search for Theories and Roles," *Political Science and Public Policy*, Austin Ranney, ed. [Chicago: Markham, 1968], pp. 151–175.) Other possibilities, of course, exist.

[62] This process of differentiation and disaggregation *without* a synthesis at a "higher" level has been noted even amongst the members of the European Community, which was recently found to be "a collection of structures rapidly growing in many directions and each very imperfectly responsive to the behavior of the others." (James A. Caporaso and Alan L. Pelowski, "Economic and Political Integration in Europe: A Time-Series Quasi-Experimental Analysis," this *Review*, 54 [June 1971], 418–433; the passage cited is on p. 433.)

and compensating for different imperfections in processes and structures of the interstate system and of states as actors in that system. The resulting international order promises to be both highly differentiated and exceedingly complex. And while we are only beginning to develop an adequate conceptual vocabulary with which to describe it, we may safely conclude that it will share virtually nothing with typical visions of "supersystems" toward which the natural logic of events is said to be propelling us.[63]

In sum, within the advanced industrial societies the shortcomings of a market-rational orientation to public decision making, in coping with highly interdependent and complex phenomena, are being experienced daily. In some sense, an analogous phenomenon is occurring within the interstate system. I have here tried to suggest a means by which we may begin to analyze this phenomenon, so as better to understand its consequences not only for the future of the international order, but for the future organization of political life in general.

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[63] I have elsewhere suggested that the devolution of organizational forms described above is resulting in a complex and fundamental reordering of political space and restructuring of public authority across states. Further, I have sought to demonstrate that just as extant theories do not adequately capture contemporary processes of international organization, so extant images do not adequately express their emergent structures. (John Gerard Ruggie, "The Structure of International Organization: Contingency, Complexity, and Post-Modern Form," *Papers, Peace Research Society International*, 18 [1972], 73–91.)

Averroës: Politics and Opinion

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In large measure the decision to classify Averroës as a neo-Platonic thinker or as a neo-Aristotelian thinker rests upon the way his explanation of the relation between politics and opinion is understood. If Averroës held opinion to be insufficient as a guide to political life, if he taught that the logical arts which are best suited to handle opinion—i.e., dialectic, rhetoric, and poetics—are to be considered no more accurate than the shadows reflected on the wall of the cave, and philosophy or theoretical knowledge to be the only means for decent political life, then he must be understood as a direct descendant of Plato.[1] Were it true, however, that Averroës considered it possible for the practical arts to be sufficiently guided by opinion, that a good political community could exist even if that community were not guided by philosophy or theoretical knowledge, then he must be considered as one who continued the thought of Aristotle.[2] Still, to begin this inquiry by drawing such a vulgar distinction is defensible only because the truly interesting problem is reflected in the vulgar distinction. In other words, the truly interesting problem is not whether Averroës continued what is usually called the Platonic tradition or the Aristotelian tradition, but what he thought of the relation between theory and practice.

For many years the issue has been debated on the basis of insufficient evidence because even though it was known that the Great Commentator wrote on almost all of Aristotle's logical and theoretical works, it has never been possible to find any evidence that he commented on the most important political work by Aristotle, i.e., the *Politics*. Instead, students of Averroës have been forced to divine the significance of his decision to comment on Plato's *Republic* because of its great similarity, in his view, to Aristotle's teaching about the subject; and such conjecture has been further complicated by the absence of the original Arabic text, which is only imperfectly represented by either the existing Mediaeval Latin and Hebrew translations or the recent English translation of the Hebrew.[3] Nor has it been possible to clarify the problem by reference to his *Middle Commentary on the Nicomachean Ethics,* for textual problems make careful study of this commentary even more difficult: it is available to us only in an awkward and often apparently erroneous Mediaeval Latin translation. Contemporary scholarship has not discovered any manuscripts of these important texts, but it has offered an alternative way of resolving the issue. New editions of the commentaries on the logical arts based on opinion and directed to action have recently become available.[4] Now it is

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[1] See Plato *Republic* 514^{a}–515^{d}, 516^{c}–517^{e}, 520^{a}–521^{b}, 543^{a}–544^{a}; *Politicus* 300^{e}; *Phaedrus* 259^{e}–261^{c}, 262^{b-c}, 270^{d}–271^{c}, $271^{d}272^{b}$, 274^{e}–275^{b}, 277^{b-c}, 278^{b-d}. Compare also al-Fārābī *Commentary on Aristotle's Rhetoric* (Cod. Paris, Bibl. Nat. lat. 16097), folios 192^{vb}–193^{ra}, as cited by William F. Boggess) "Alfarabi and the Rhetoric: the Cave Revisited" in *Phronesis,* 15 (1970), 89.

[2] See Aristotle *Politics* $1288^{b}10$–$1289^{a}25$ (IV. i. 1–6): *Nicomachaen* Ethics $1094^{a}1$–$1095^{a}13$ (I.i–iii), $1180^{a}15$–$1181^{b}23$ (X. ix. 11–23). See also Muhsin Mahdi, *Ibn Khaldūn's Philosophy of History* (Chicago: Univ. of Chicago Press, 1964), pp. 274–275.

[3] For the decision to comment on Plato's *Republic,* see *Averroës' Commentary on Plato's Republic,* ed. and trans. E. I. J. Rosenthal (Cambridge: Univ. Press, 1966), p. 112 (First Treatise, Chapter 1, Sections 6–8, according to Rosenthal's editing). Averroës made his judgment about the similarity between the book by Plato and that by Aristotle with full knowledge that Aristotle had not perceived such a similarity. Compare Averroës, *Libros Decem Moralium Nicomachiorum Expositio* in *Aristotelis Opera cum Averrois Commentariis* (Venice: Apud Junctas, 1562) Vol. III, folio 160 G.

[4] Averroës considered Aristotle's *Organon* to consist of eight books: *Categories, De Interpretatione, Prior Analytics, Posterior Analytics, Topics, On Sophistry, Rhetoric,* and *Poetics.* According to him, these eight books present five logical arts: demonstration, dialectic, sophistry, rhetoric, and poetics. The syllogisms proper to the art of demonstration are constructed with apodeictic premises; consequently, when they are correctly constructed, they necessarily lead to certain conclusions. However, the arguments used in the arts of dialectic, rhetoric, and poetics are constructed with premises derived from opinion; consequently, even when they are correctly constructed, they do not necessarily lead to certain conclusions. Because sophistry aims at deceiving only for the selfish gain of the speaker, the logical character of its premises is not an important consideration.

In recent years ʿAbd al-Raḥmān Badawī has edited the *Middle Commentary on Aristotle's Rhetoric* or *Talkhīṣ al-Khaṭābah* (Cairo: Maktabat al-Nahḍah al-Miṣrīyah, 1960) and the *Middle Commentary on Aristotle's Poetics* or *Talkhīṣ al-Shiʿr;* the latter work appeared in *Arisṭūṭālīs Fann al-Shiʿr* (Cairo: Maktabat al-Nahḍah al-Miṣrīyah, 1953), a volume containing the Arabic translation of Aristotle's *Poetics* and editions of short commentaries on the *Poetics* by al-Fārābī and Avicenna. Salīm Sālim has also edited the *Middle Commentary on Aristotle's Rhetoric;* see *Talkhīṣ al-Khaṭābah,* ed. Salīm Sālim (Cairo: Dār al-Taḥrīr li al-Ṭabʿ wa al-Nashr, possible, therefore, to work towards an understanding of this problem by something similar to the method of induction, that is, by gathering the elements constitutive of a generalization through judicious appraisal of what Averroës said about the logical arts, opinion, and the practical arts, as well as about the relation among all of these. In addition, there have been recent editions of Averroës's admittedly public works, works in which he presented a political teaching in his own name. These works provide a description of the rhetorical, dialectical, and theoretical teaching appropriate to each class of people, as well as suggestions about the relationship between each kind of teaching and decent political life. Careful attention to the view of politics which guides these suggestions and to the kind of reasoning Averroës used in these works (e.g., the *Epistle Dedicatory, Decisive Treatise, Uncovering of the Clear Paths,* and *The Incoherence of the Incoherence*) will make it possible to approach an understanding of the problem by a method similar to deduction.[5] On the basis of all of these works, then, it now seems that an understanding of the way Averroës thought theory should influence practice is within reach.

Dialectic and rhetoric are based on opinion; differently stated, the premises used in them are derived from opinion, not from certainty. Yet the quality of opinion on which the premises in each art rest and the rigor of the reasoning used to bring about a conclusion in each art differ to such an extent that the conclusions of each have varying proximate relationships to certainty. Consequently, each art may be used more appropriately for certain kinds of inquiry and for certain kinds of persuasion. In his commentaries on these arts, it was to such questions that Averroës invariably addressed his first thoughts. Nonetheless, the *Short Commentaries* on each art are not always sufficient guides to the full teaching; they sometimes need to be supplemented by the larger commentaries on each art, by commentaries on other logical arts, or even by commentaries on other subjects in order to discern the full perspective in which Averroës viewed them.

For example, in the *Short Commentary on Aristotle*'s *Topics,* dialectic is said to have premises based on widespread, well-known opinion, to provide belief approximate to certainty, and as a result, to be closer to the art of demonstration than rhetoric or poetics. It is nowhere indicated there, however, that dialectic may consequently be used to inquire into the theoretical arts, or, more precisely, into the same subjects as first philosophy, i.e., metaphysics. In that commentary only the manner in which dialectic may be used as a tool of exercise is mentioned; the indication of the subjects into which dialectic may inquire must be found in other commentaries.[6] Silence about the subjects of inquiry proper to dialectic is accompanied by silence about those who would most likely be considered practitioners of dialectic: no mention is made of the dialectical theologians (*mutakallimūn*), and no examples of the good or bad use of dialectic are explicitly attributed to them. This latter silence, coupled with the strange reticence of Averroës about the theoretical subjects dialectic might investigate, would seem to suggest that he wanted to lead the reader to understand that by insisting on using the art only for such investigations the dialectical theologians did not really have any knowledge about the art. Such an impression is con-

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1967). The *Short Commentaries on Aristotle's Topics, Rhetoric, and Poetics,* which heretofore could only be consulted in two Judaeo-Arabic manuscripts (Cod. Paris., Bibl. Nat. Or., 1008 and Cod. München., König. Hof. und Staats Bibl. arab., 650^{a}) and in Mediaeval Latin translation, have now been edited and translated into English. They will soon be published under the title *Averroës' Three Short Commentaries on Aristotle's Topics, Rhetoric, and Poetics* (New York: SUNY Press, 1972).

[5] The *Decisive Treatise* or *Book of the Decisive Treatise and Stipulation of the Relationship between Divine Law and Philosophy* and the *Epistle Dedicatory* have been edited and translated by George F. Hourani; see *Kitāb Faṣl al-Maqāl wa Taqrīr mā bayna al-Shari‘ah wa al-Ḥikmah min al-Ittiṣāl* (Leiden: E. J. Brill, 1959) and *Averroës on the Harmony of Religion and Philosophy* (London: Luzac and Co., 1961). For the decision to call the *Ḍamīmah Epistle Dedictary,* see Mushin Mahdi, "Averroës on Divine Law and Human Wisdom" in *Ancients and Moderns,* ed. Joseph Cropsey (New York: Basic Books, 1964), pp. 114–131. *The Uncovering of the Clear Paths* or *The Book of Uncovering the Clear Paths of the Signs about the Beliefs of the Religious Community* was recently edited by Maḥmūd Qāsim; see *Kitāb al-Kashf ‘an Manāhij al- ’Adillah fi ’Aqā‘id al-Millah,* ed. Maḥmūd Qāsim (Cairo: Maktabat al-Anglū al-Miṣrīyah, 1963). Simon van den Bergh has translated *The Incoherence of the Incoherence* from Father Bouyge's earlier edition of the *Tahāfut al-Tahāfut;* see *The Incoherence of the Incoherence,* trans. Simon van den Bergh (London: Luzac and Co., 1954).

[6] References to the *Short Commentaries on Aristotle's Topics, Rhetoric, and Poetics* are to the paragraphs of the forthcoming edition. Compare Averroës, *Short Commentary on Aristotle's Topics,* para. 21; *Jāmi‘ Kitāb al-Burhān* [*Short Commentary on Aristotle's Posterior Analytics*]. Cod. Paris., folios 62^{a}15–63^{a}6, Cod. München., folios 56^{b}22–57^{a}24; *Talkhīṣ Kitāb mā ba‘da al-Ṭabī‘ah* [*Middle Commentary on Aristotle's Metaphysics*], ed. ‘Uthmān ‘Amīn (Cairo: Muṣṭafā al-Bābī al-Ḥalabī, 1958), pp. 1:10–2.8, 5:12–6:14. Contrast these with Aristotle *Posterior Analytics* 77^{a}29–35, 86^{a}22–30.

firmed by the fact that the one argument he most explicitly condemned as a poor use of dialectic is the argument about the creation of the world, a view central to their doctrine.[7]

In the *Short Commentary on Aristotle's Rhetoric,* however, examples of arguments attributed to the dialectical theologians and censure of their procedure abound. Apparently Averroës wanted to suggest that they were really rhetorical theologians—and bad practitioners of the art of rhetoric at that. This longstanding confusion about the practitioners of the two arts might have arisen because of the close similarity between the art of dialectic and the art of rhetoric. To show the limitations of that similarity, Averroës explained that the conclusions of rhetoric are derived from premises based on prevailing unexamined opinion and are therefore no more than persuasive. This means that in rhetorical argument people are induced to believe the conclusions for reasons that do not usually withstand deeper scrutiny.[8] Since dialectical arguments can withstand deeper scrutiny and are based on sounder premises, it is evident that the two arts and their practitioners are distinct.

This reasoning becomes more compelling when it is noted that the character of the conclusions which are derived from rhetoric makes it most useful when time or mental incapacity prevents people from closely examining them. For similar considerations, the arguments leading to those conclusions are not complicated or difficult. Thus, rhetoric is most appropriately used in the activities for which people ususally use it—interpersonal dealings and political matters.[9] By extending that reasoning, it would seem that rhetoric could not be put to the same investigative uses as dialectic with any expectation of obtaining sound results. Averroës apparently did not wish to draw that consequence, however, for he suggested in a different context not only how rhetoric might be used by others to communicate certain religious and metaphysical ideas to the people, but also how the people themselves might use it to inquire into such ideas on their own.[10]

So despite the technical limitations placed on both arts in the *Short Commentaries,* the broader view seems to be that both dialectic and rhetoric are very similar with respect to the scope of their inquiry. From this broader perspective, then, the decisive difference between the two arts is only minimally the extent to which their conclusions approximate certainty, that is, the extent to which the premises of each art are based on opinions having greater or lesser certainty. The broader perspective makes it necessary to distinguish the two arts on the basis of use. Indeed, that is precisely the consequence of Averroës's insistence that the truly decisive difference is the relationship of each to sophistry: dialectic is fundamentally different from sophistry, whereas rhetoric may at times be identical to it in everything but purpose.[11] This means that he considered it possible for a certain amount of deceit to accompany rhetorical argument without the conclusion being less rhetorical. That is because the mental capacity of the audience or the time limitations imposed upon rhetorical discourse might necessitate the introduction of uncomplicated—but deceptive—arguments. Dialectical argument, however, is never deceptive and therefore often quite complicated. For that reason he considered it generally unsuitable when the mental capacity of the audience or time limitations were important factors. In short, by insisting that the decisive difference between rhetoric and dialectic was ultimately the relation of each to sophistry, Averroës emphasized the limitations of dialectic for public discourse and the possibilities of using rhetoric to present the substance of dialectical arguments in a popular manner.

Apparently, none of this was understood by the dialectical theologians; or it was understood, but ignored for base reasons.[12] Whatever the reason, the dialectical theologians' confusing of the two arts brought great harm to Muslim political communities in that their use of complicated arguments for instructing the people about religious doctrine was more a cause of misunderstanding than of enlightenment. Religious schisms arose, and the more zealous sought to gain acceptance of their ideas by force. By Averroës's time, political faction, intrigue, and general strife were widespread.

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[7] Averroës, *Short Commentary on Aristotle's Topics,* paras. 6–8.

[8] Averroës, *Short Commentary on Aristotle's Rhetoric,* paras. 2–4, 12, 29, 31, 36, 38, 40, 42–44; *Talkhīṣ al-Khaṭābah* [*Middle Commentary on Aristotle's Rhetoric*], Badawī edition, pp. 3:5–4:4, 20:9–22:9; *Short Commentary on Aristotle's Topics,* para. 2.

[9] Averroës, *Short Commentary on Aristotle's Rhetoric,* para. 45; *Talkhīṣ al-Khaṭābah* [*Middle Commentary . . . Rhetoric*], Badawī edition, pp. 4:5–16, 28:9–29:5.

[10] Averroës, *Uncovering of the Clear Paths,* pp. 153:5–154:15 (pagination of Müller edition is pp. 45–47).

[11] Averroës, *Talkhīṣ al-Khaṭābah* [*Middle Commentary . . . Rhetoric*], Badawī edition, pp. 13:5–15.8.

[12] *Talkhīṣ al-Khaṭābah* [*Middle Commentary . . . Rhetoric*], Badawī edition, pp. 10:14–21, 18:10–12.

Philosophical thought, as well as religious speculation, were threatened.

Averroës seemed to have attempted to correct the basic error by the way he presented his *Short Commentaries* on the dialectical and rhetorical arts. He first separated what had been falsely joined; then by suggesting the conditions needed for dialectic proof, failing to mention the dialectical theologians specifically, and yet citing a favorite argument of theirs as a bad example of dialectical reasoning, he showed that they could not properly be classed as practitioners of dialectic. Finally, his references to the dialectical theologians and to their arguments in the course of his discussion of rhetoric suggested that they were really practitioners of this art, while his censure of them showed that they were faulty practitioners of the art; at the same time, his exposition of the conditions for sound rhetoric showed how their faults could be avoided. The parallel drawn between rhetoric and sophistry in the rest of his work complemented the argument of these two commentaries and pointed to a new use of rhetoric as the basic cure. Neither the criticism nor the remedy exceeded the limits of opinion: Averroës merely insisted upon a stricter accounting of these arts in accordance with what was generally known about them.

The different emphasis on the way these arts should be used, which is more fully developed in the rest of his teaching, first becomes evident in the *Short Commentary on Aristotle's Poetics*. In the other two *Short Commentaries*, the art of dialectic was described as best used between two investigators of fairly equivalent capabilities and the art of rhetoric as best used by the multitude among themselves or by someone skilled in the art in order to speak with the multitude on certain issues. In the *Short Commentary on Aristotle's Poetics*, this art is described as best used to move the souls of the multitude to believe or disbelieve, to do or refrain from doing, something.[13] In other words, the art of poetics is used by one or a few people and is directed toward the multitude. Whereas the teaching in the *Short Commentary on Aristotle's Rhetoric* stressed the technical aspects of the art of rhetoric and remained vague about who would use the corrected art or exactly how it would be used, the teaching in the *Short Commentary on Aristotle's Poetics* stressed the way the art of poetics would be used more than how one might explain it. Even though the necessity for training in the arts of dialectic and of rhetoric was stressed in both the *Short Commentary on Aristotle's Topics* and the *Short Commentary on Aristotle's Rhetoric,* there was no insistence in either—and especially not in the *Short Commentary on Aristotle's Rhetoric*—that someone well skilled in the particular art would use it to influence the multitude. From the very beginning of the *Short Commentary on Aristotle's Poetics,* however, a distinction between the few, i.e., those skilled in the art of poetics, and the many or the multitude is massively present.

The significance of that distinction is heightened by the absence of any indication about how the practitioner of this art arrives at the similitudes by which he induces the many to do or to believe something. Although it is admitted that the similitudes may be erroneous, this shortcoming is allegedly not as important as the advantages the art of poetics offers in inducing the many to belief or action.[14] In another context, a failure to explain the logical background of poetical similitudes would not be so important. In this context, it does occasion further thought. First of all, the best known poetic work—the *Qur'ān*—looms large in the background of any Muslim community. Moreover, it would never be simply false to speak of the *Qur'ān* as a poetic work which seeks to induce people to believe or to do certain things. But that poetic work is considered to be unique, as is the poetic skill of its author. Does the *Short Commentary on Aristotle's Poetics* mean to suggest that the *Qur'ān* can be imitated in some way, or that the skill of its author can be duplicated by some human? Apparently, the silence about how someone acquires poetic skill—a silence not found in either of the other two commentaries—is meant to make such a conclusion difficult to prove; nonetheless, it cannot be denied that the commentary does permit that conclusion to be drawn.

If this surmise is warranted, then the man who would properly use the art of poetics would be no ordinary man and would have no ordinary understanding of things. To a certain extent, it might be argued that the position of this commentary among a group of *Short Commentaries* on the logical arts already assumes that no ordinary man will be concerned about the art of poetics from the perspective of its political value. Not only does the study of such a group of works demand great intellectual toil, it promises to prepare those who study it to attain true theory—something that interests few men and something for which even fewer might profitably strive.[15] Only philosophers or those interested in philosophy seek true theory or seek to prepare themselves to attain it.

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[13] Averroës, *Short Commentary on Aristotle's Poetics,* paras. 1, 4.

[14] *Short Commentary . . . Poetics,* paras. 2–3.

But does such a promise mean to allude to the notion that good practice of these arts depends on true theory? Or does it mean to suggest that only those who have true theory can use these arts in the way they are supposed to be used? In principle, one learns the logical arts in order to consider other arts, especially the theoretical arts, and common experience teaches that the nontheoretical arts can be practiced without knowing logic.[16] But now it seems that serious men ought to learn about all of the logical arts in order to prepare themselves to practice well some of the logical arts, e.g., dialectic, rhetoric, and poetics. Although these arts—no less than the art of politics—can be practiced without a knowledge of formal logic, they do depend on logic; that is, they are either part of the logical inquiry or they use reasoning that resembles the syllogism and induction used in the more formal or rigorous logical arts. That dependence means that those who seek to use these arts can derive most benefit from them through prior familiarity with the formal logical arts. Thus, there was an important practical consideration behind Averroës's insistence on the need for the practitioners of rhetoric to learn formal logic and behind his extremely basic explanation of logical issues in the *Short Commentary on Aristotle's Rhetoric*. Differently stated, criticism of the usual practitioners of rhetoric—the dialectical theologians—as ignorant of the art of logic could only have been secondary, and the more profound reason must have been that he wished to emphasize how essential a knowledge of logic was to the sound practice of rhetoric. The very important political use to which rhetoric can be put now takes on new significance.

Still, to conclude that Averroës was apparently unwilling to admit that politics could be practiced decently without something like theoretical knowledge to guide it is too premature. Thus far, Averroës can be said to have done little more than prove that when rhetoric or dialectic is used by those who have no knowledge of either art, only confusion, misguidance, and harm can result. Although awareness of the logical arts, even knowledge of them, was presented as the remedy for that evil, it could never be presented as a sufficient guarantee of decent political life. Awareness of the logical arts, even knowledge of them, is not theoretical knowledge in that most important sense. First of all, knowledge of logic is by all accounts only propaedeutic to broad theoretical knowledge. Secondly, the practical arts are concerned with substantive issues; and procedural knowledge—that is, the kind of knowledge arising from the study of logic—has always been insufficient to deal with such issues. But for these very reasons, even if knowledge of logic is not a sufficient guarantee of the most important theoretical knowledge, it is a necessary condition of such knowledge.

That is still not an adequate consideration, however, because at least two of the logical arts based on opinion are usually practiced by men having little awareness of the fuller logical context from which these arts are derived. Knowledge of all of the logical arts would permit these men to pursue their own arts in a more proficient manner and would teach them how to avoid serious errors in the conclusions they try to reach with their arts, but such knowledge would not teach them which of two equally sound conclusions is practically better. In everything which preceded the *Short Commentary on Aristotle's Poetics*, Averroës seemed content with such limitations: if the practitioners of dialectic and rhetoric knew the logical limitations of their arts, they would not make the mistakes that other practitioners of these arts had made, further instruction in the substantive areas which these arts covered did not seem necessary. It was only in the *Short Commentary on Aristotle's Poetics* that the necessity for something more than technical knowledge appeared: but the precise nature of this "something more" was left vague.

Because the art of poetics was to be used to induce the multitude to belief and action, it is not sufficient to suggest that the "something more" is only the inspiration of the Muses. Because the "something more" cannot be so easily explained away, the teaching of Averroës about the logical arts is very important. The *Middle Commentaries*, and especially the *Middle Commentary on Aristotle's Rhetoric*, suggest that this "something more" is philosophy. There are strong indications in the *Middle Commentary on Aristotle's Rhetoric* that Averroës considered the man who masters the logical arts to be identical with the philosopher.[17] Since he frequently referred to the philosophers as knowers, rather than simply seekers, that identification suggests that theoretical knowledge is the ultimate guide. However, the indication is left as vague as it was in the *Short Commentary on Aristotle's Poetics.* The indication itself could be explained as a concession to the dominant opinion of this time: because Muḥammad as prophet-rhetorician represented the ideal of rhetoric and was obviously wise, most people would agree that he possessed the qualities required of a philosopher or a master of logic. However, the indication is more than a concession to the dominant opinion of the day because it contains a very intriguing suggestion: if Muḥammad's rhetorical skill rests on bringing knowledge of the logical arts and theoretical knowledge together to provide for sound practice, then that skill or that unique understandng is within the reach of other humans. Still, Averroës's deliberate vagueness about these matters cannot be ignored. The vagueness alone suggests that he did not wish to go beyond the practical limits of the position suggested in the *Short Commentary on Aristotle's Topics* and he *Short Commentary on Aristotle's Rhetoric,* while the hint of something more in the *Short Commentary on Aristotle's Poetics* and in the *Middle Commentaries,* suggests that he wanted to leave the possibility open for letting theoretical knowledge guide practice.

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[15] *Short Commentary . . . Poetics*, para. 5.

[16] See Averroës, *Jāmiʿ Kitāb al-Burhān* [*Short Commentary . . . Posterior Analytics*], Cod. Paris., folio 62^{a}6–15, Cod. München., folio 56^{b}1–22; *Taṣdīr ʿAmm (General Introduction to the Short Commentaries on Aristotle's Organon)*, Cod. München., folio 1^{a}1–1^{b}17.

[17] See Averroës, *Talkhīṣ al-Khaṭābah* [*Middle Commentary . . . Rhetoric*] Badawī edition, pp. 9:19–10:8, 249:16–20.

Students of Aristotle will have noticed already that from his beginning remarks about dialectic, Averroës took great liberty with Aristotle's thought in his commentaries on Aristote's logical writings. These liberties culminate in the teaching that unexamined opinion is an insufficient guide for the arts which are based on unexamined opinion. But Averroës did not go so far as to claim that such arts can be guided solely by theoretical knowledge; he only insisted that dialectic and rhetoric can best be used by those who have thought about the limitations of opinions—that is, by men who are massively aware of their own ignorance. As uch, this insistence is little more than an interpretative extension of Aristotle: the implications hat Aristotle would endorse this notion arise rom the fact that he deemed it worthwhile to write on these subjects in order to instruct people about the limitations of opinion. But the departure from Aristotle is most marked in Aveoës's description of the way to reform rhetoric nd his account of the uses of poetics, for Aristotle's treatment of rhetoric allows rhetoric to be improved by study of his treatment of that rt alone without studying the other logical arts, nd his treatment of poetics never suggested that it needed to be guided by another art. Thus, Averroës went somewhat beyond Aristotle by teaching that something more than opinion was needed for sound practice, but he did not go as far as Plato and insist that only theoretical knowledge could guide such practice.

As was previously stated, these arguments have been directed from something approaching an inductive point of view. The facts of the matter were considered in detail and an attempt made to bring them together in a coherent manner. Another way of looking at the problem —something similar to a deductive procedure —has yet to be considered. By starting with the way Averroës used these arts, it may be possible to deduce the kinds of premises on which his use of them is based.

That the *Epistle Dedicatory, Decisive Treatise,* the *Uncovering of the Clear Paths,* and the *Incoherence of the Incoherence* are dialectical works may be shown by the following considerations. The reasoning used in each is too complicated to be rhetorical: none of the syllogisms conforms to the standards set down for rhetorical syllogisms or enthymemes; similarly, none of the examples is as clear as is required for rhetorical examples. The reasoning in each book, however, is based on what is generally accepted and is explicitly admitted to be nondemonstrative. By process of elimination, then, the books must be dialectical. Further evidence of their dialectical character is that they are addressed to one person and proceed by steps similar to those used by a questioner and an answerer. Neither the legal setting of the *Decisive Treatise* (which suggests that it is a book in which forensic rhetoric would be the appropriate method of address) nor the advice given to one person in all four works (which suggests that they are examples of deliberative rhetoric) is sufficient to refute the contention that they are examples of the dialectic art, as long as the technical character of the syllogism is considered.

As has been noted, these four works are Averroës's practical works, the works in which he set forth—in his own name—his views about political, philosophical, and religious issues. All three issues come together in the one problem that loomed largest before him: redeeming philosophy from the religious and political disfavor into which it had fallen. To succeed in that goal meant resolving the political and religious strife which gave rise to the probelm. The most obvious aspect of his redemptive action has always been superficially recognized, but its import seems thus far to have been ignored. In all four works, Averroës stressed the usual distinction between the demonstrative, dialectical, and rhetorical class of people, as well as the need to speak to each in the manner which each may understand.[18] Yet, with few exceptions, he limited his discussion of how this should be done to explanations pertinent to the demonstrative class and the rhetorical class alone. Averroës explained how the multitude ought to be addressed and how the learned men, the people of demonstration, ought to speak among themselves. The effect of his teaching is to entrust the learned men with all the practical decisions for the multitude, as well as for themselves. This dichotomy is in no way alleviated by the tripartite division between the demonstrative, dialectical, and rhetorical classes of people, for there is no indication in any of these works about the kind of knowledge which ought to be appropriate to dialectic—none, that is, save the content of each of these works. In the absence of any other indication, one can only infer that dialectic can best be used as a defensive tool to save philosophy and religion. It is a defensive weapon to be kept in the hands of the learned men or philosophers and removed from the hands of those who have heretofore misused it, the dialectical theologians.

The consequence of that distinction is that the reforms proposed in these works operate on two levels. In the first place, they provide an outline or a suggestion of how the best regime could be ordered. In the best regime, the learned men would direct everything: in that regime, the meaning of the law would be understood by them, interpreted among themselves, and explained to the people by means of sound rhetoric; dialectic would either be used to train those who aspire to the class of the learned or to explain particular matters to those few who go beyond the level of the multitude but fall short of the level of the learned (*a priori*, there is no reason why this latter class of people might not be the ruling class—a ruling class directed by the learned class); and all aspects of speech would be informed by the understanding obtained by those who are masters of logic, the philosophers. Hence, in the best regime, practice would depend upon the wisdom of those who seek a correct theoretical understanding of all things. Fidelity to the text, however, demands a stronger definition of philosophers than seekers, for Averroës often referred to the philosophers as knowers. There is, then, a very strong possibility that the best regime would be based on theoretical knowledge of the whole.

Still, it is not necessary to raise the question of the best regime in these works. The reforms suggested by Averroës can be explained on a more immediate level. By insisting on the division of people into the three classes of demonstrative, dialectical, and rhetorical, he was able to defend philosophy from the attacks of overzealous traditionalists and from those who failed to understand the barriers a philosophic man would want to erect around philosophy, as well as to protect the multitude from confusion about the most important matters of faith. These important concerns led him to explain the relevance of Greek thought to the Muslim community. In the first place, this meant that the manner in which the people were spoken to had to be consonant with their ability to understand: short, unambiguous statements had to be used to persuade them. Subsequently, more complex reasoning could be used to address the more learned, that is, people like the rulers. This same kind of reasoning could be used to fence with those people who considered themselves learned and whose own confusion led to popular confusion. Similarly, on the basis of the older Greek view about the limitations of human reason, a better defense of the mysteries of the Divine Law could be made, the studies of the truly learned defended, and the attacks of the dialectical theologians thrust aside. Once a resting place for theoretical study was secured, little needed to be said about the results of theoretical investigation: as long as it could be shown in time of need that theoretical study did not threaten the community, no more was needed.

To the extent that this description of the practical aspect of Averroës's teaching is accurate, it points to the necessary relationship between the substance of what is taught to the multitude and the defense of theoretical study: in order to protect the possibility of philosophy, philosophers will always have to be concerned with popular rhetoric. Differently stated, Averroës thought that a defense of philosophy required the philosophers to assure the correct political and religious education of the people by carefully instructing the ruler in these matters through the art of dialectic. Precisely because that instruction is conducted according to dialectical premises, it is not clear whether theoretical knowledge needs to direct practice. All that is required on the most immediate level is that the possibility be maintained, for it can be persuasively argued that practice works best when guided by theory.

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[18] See Averroës, *Decisive Treatise*, pp. 6:15–7:16, 14:18–18:19, 19:10–24:8, 24:14–26:2; *Uncovering of the Clear Paths*, pp. 150:5–154:15 (Müller pagination is pp. 43–47), 157:6–159:9 (Müller: 48–50), 162:3–9 (Müller: 53), 165:13–167:14 (Müller: 56–57), 173:14–174:2 (Müller: 62–63), 178:10–180:19 (Müller: 67–69), 182:3–185:2 (Müller: 70–73), 193:1–194:17 (Müller: 79–80), 240:1–241:4 (Müller: 118–119), 249:8–251:14 (Müller: 125–127); *Incoherence of the Incoherence*, pp. 207:3–209:9, 356:7–358–12, 427:9–430:15, 514:9–515:2, 527:2–528:5.

Thus, according to Averroës, it was not necessary that practice be informed by theory to be sound. To be sure, the errors arising from unsound practice could be traced to ignorance—not simply to ignorance of correct theory, but more directly to ignorance of the limits of the arts and to ignorance of basic facts of human limitations. To remedy these errors, little more than proper instruction in the logical arts and in the basic types of human capacities was needed. Such instruction presupposed no all-pervasive view of the universe; rather, it was primarily inductive and proceeded from the observation of language conventions to the formulation of generalizations about the conditions for different kinds of investigations. That was all that was needed at the moment, and Averroës sought the most direct solution to the problem—a solution that forced his opponents to concede the necessity of philosophy and its role in the direction of sound practice. But he also understood that theoretical knowledge would indeed be the best guide to sound practice and tried to avoid any argument that would deny such a position. That he did not stress the argument more explicitly in any of these works suggests that he did not think it necessary to stress such an obviously true proposition and that he preferred to remedy the evils of what he saw around him in the easiest possible way, perhaps just as much in order to devote his time to the more important quest for theoretical knowledge as in order to assure the acceptance of his practical reforms by making them as compelling as possible.

Integration and Instability: Patterns of African Political Development*

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A pervasive concern of political theory has been the explanation of order, however defined, and there has been recently considerable interest in theoretical and empirical approaches to the analysis of political integration and instability. Integration has been one of the more dynamic research foci, particularly in the study of international relations, while the study of political instability has seemed to dominate much recent comparative, or cross-national, research in political science.[1] We attempt in this paper to make some contribution to the synthesis of theoretical work on integration and instability; to formulate a testable model of national political integration and instability, and to analyze the empirical relationships between patterns of integration and instability in the "new" states of Sub-Saharan Africa.[2]

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[1] The work of Karl Deutsch represents some of the major explorations in the theory of political integration, and his thinking is well represented in his contributions to the following works: *International Political Communities—An Anthology* (New York: Doubleday Anchor Books, 1966), and Philip Jacob and James V. Toscano, eds., *The Integration of Political Communities* (New York: J. B. Lippincott Co., 1964). Both of these works include other useful material on the subject. Also useful are: Amitai Etzioni, *Political Unification: A Comparative Study of Leaders and Forces* (New York: Holt, Rinehart and Winston, Inc., 1965); Johan Galtung, "A Structural Theory of Integration," *Journal of Peace Research,* 4 (1968), 375–395, and Claude Ake, *A Theory of Political Integration* (Homewood, Illinois: Dorsey Press, 1967), which is one of the few recent works specifically concerned with national integration. Also see Leonard Binder, "National Integration and Political Development," *American Political Science Review,* 28 (September, 1964), 622–631; Myron Weiner, "Political Integration and Political Development," *The Annals,* 358 (March, 1965), 52–64; Ralph M. Goldman, "A Transactional Theory of Political Integration and Arms Control," *American Political Science Review,* 63 (September, 1969), 719–733; and Arend Lijphart, "Cultural Diversity and Theories of Political Integration," *Canadian Journal of Political Science,* 12 (March 1971), 1–14.

Representative of some of the recent work on political instability are: D. P. Bwy, "Political Instability in Latin America: The Cross-Cultural Test of a Causal Model," *Latin American Research Review,* III (1968), 17–66; James C. Davies, "Political Stability and Instability: Some Manifestations and Causes," *Journal of Conflict Resolution,* 13 (March, 1969), 1–17; Ernest A. Duff and John F. McCamant, "Measuring Social and Political Requirements for System Stability in Latin America," *American Political Science Review,* 62 (December, 1968), 1125–1143; Ivo K. and Rosalind L. Feierabend, "Aggressive Behavior Within Politics, 1948–1962: A Cross-National Study," *Journal of Conflict Resolution,* 10 (September 1966), 249–271; Egil Fossum, "Factors Influencing the Occurrence of Military Coups d'Etat in Latin America," *Journal of Peace Research,* 3 (1967), 209–227; Ted Gurr, "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review,* 63 (December, 1968), 1104–1124, and *Why Men Rebel* (Princeton, New Jersey: Princeton University Press, 1970); Manus Midlarsky and Raymond Tanter, "Toward a Theory of Political Instability in Latin America," *Journal of Peace Research,* 3 (1967), 209–227; Sten Sparre Nilson, "Measurement and Models in the Study of Stability," *World Politics,* 11 (October, 1967), 83–110; Taketsugu Tsurutani, "Stability and Instability: A Note in Comparative Political Analysis," *The Journal of Politics,* 30 (November, 1968), 910–933; and Aristide Zolberg, 1968, "The Structure of Political Conflict in the New States of Tropical Africa," *American Political Science Review,* 62 (March, 1968), 70–87. The most recent evidence of the interest in the study of instability is in the work generated by the National Commission on the Causes and Prevention of Violence. See especially the comprehensive historical and comparative treatment in *The History of Violence in America,* ed. Hugh Davis Graham and Ted Robert Gurr (New York: Bantam Books, 1969).

[2] Because the statistical generalizations on which the analysis is based make the discussion very anonymous, it will be useful to bear in mind that the countries for which data have been gathered are: Bot-

The basic proposition of the theory outlined here is that increasing integration decreases the likelihood of instability.[3] Integration is the degree of cohesion that binds members of social systems together, and is generally thought of in terms of the values, institutions and communications which facilitate escalating sequences of social contact, cooperation and consensus. Instability is a condition in which conflict in social systems is no longer adequately regulated and results in a rapid and relatively extreme change in the structure of the system. Instability is, therefore, generally thought of in terms of violent confrontation between actors in the political system, or better, as activity that exceeds the normative and behavioral patterns that are regulated by the institutional order.[4]

This paper attempts (1) to give operational specificity and clarity to the umbrella concepts of integration and instability; (2) to state the theoretical linkages between different dimensions of integration and instability; and (3) to evaluate this theory with data from contemporary African political development.[5] The following analysis is part of a larger work[6] on the factors affecting political conflict in Africa, in which we isolate dimensions of conflict between different political actors; the socioeconomic processes that exacerbate such conflict, and the integrative processes that contain or resolve political conflict. Although any adequate explanation of political instability must deal both with the generation and with the resolution of conflict,[7] this paper is concerned only with the latter problem and the theoretical expectation that integration modifies conflict and inhibits instability in political systems. If this is only half the picture, it is a subject that is theoretically imprecise and inadequately tested in cross-national research.

Theory and Operationalization

If the theory of integration is to produce any explanation of processes of national political instability, one has to be able to say in what ways a nation is more or less integrated, and to distinguish these descriptions sharply from the conceptual and operational meanings of political instability. In general systems terms, integration may be defined as "a relationship among units in which they are mutually interdependent and jointly produce system properties which they would separately lack."[8] In such general terms, however, there is a danger of redundant or tautological usage of the terms "system" and "integration." Attempts at more specific definition run the contrasting risk of tautological association between integration and

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swana, Burundi, Cameroon, Central African Republic, Chad, Congo (Brazzaville), Zaire, Dahomey, Ethiopia, Gabon, Gambia, Ghana, Guinea, Ivory Coast, Kenya, Lesotho, Liberia, Malawi, Mauritania, Mali, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Somali Republic, Sudan, Tanzania, Togo, Uganda, Upper Volta, Zambia.

[3] The fundamental proposition relating integration to instability is summed up by Mack and Snyder in their review of conflict theory: "The more integrated into the society are the parties to conflict, the less likely will conflict be violent. . . . Basically, the stability-instability balance will be a resultant of the success or failure of the normative order in regulating conflicts of interest." Raymond W. Mack and Richard C. Snyder, "The Analysis of Social Conflict—Toward an Overview and Synthesis," *Journal of Conflict Resolution*, 1 (June, 1957), 227.

[4] See the contrasting definitions of violence in Chalmers Johnson, *Revolutionary Change* (Boston: Little, Brown, 1966), p. 8, and H. L. Nieburg, *Political Violence* (New York: St. Martins Press, 1969), p. 13. It should be noted that structural violence as opposed to interpersonal and physical violence is characteristic of the most stable political systems. See the distinctions made in Johan Galtung, "Violence, Peace, and Peace Research," *Journal of Peace Research*, 3 (1967), 167.

[5] Quantitative, cross-national research on political development has for the most part ignored African cases, for the very intelligible reason that adequate aggregate data sources for these new nations have not been readily available. The data used in this paper were collected by the African Integration Project at Northwestern University. A complete African data bank is being prepared for circulation on tape, and selections from those data, with discussions of conceptual and methodological issues related to the use of the data, are presented in Donald G. Morrison, Robert C. Mitchell, John N. Paden and Hugh Michael Stevenson, *Black Africa: A Handbook for Comparative Analysis* (New York: The Free Press, 1972). Sources for the data used in this article are detailed in that work, and most of the indicators used here are presented in rank-ordered tables by nation in the book. Readers interested in additional details relating to the sources of the data in this article are invited to communicate with the authors c/o the Institute for Behavioral Research, York University, Downsview, Ontario.

[6] See Donald G. Morrison and Hugh Michael Stevenson, *Conflict and Change in African Politics* (New York: The Free Press, forthcoming).

[7] "Any attempt to measure the likelihood of revolution should . . . encompass the social sources of strains and tensions present in the several societies and the social conditions that either encourage or balance extreme acts among the contending forces . . . the likelihood of revolutions is a function of both positive or conflict-generating factors and negative or conflict-controlling factors." Arnold Feldman, "Violence and Volatility: The Likelihood of Revolution," in *Internal War, Problems and Approaches*, Harry Eckstein, ed. (New York: The Free Press, 1964), p. 115.

[8] Karl W. Deutsch, *The Analysis of International Relations* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968), pp. 332–40.

stability, as, for example, in the following well-known definition:

> By integration we mean the attainment, within a territory of a "sense of community" and of institutions and practices strong enough and widespread enough to assure, for a "long time," dependable expectations of "peaceful change" among its population.[9]

In order to resolve the ambiguous identification of integration and stability suggested by passages such as this, we have attempted to construct a theory in which separable dimensions of integration are related to, but not identical with instability. We identify four dimensions of national integration: horizontal integration, vertical integration, value integration, and centralization.[10]

Horizontal integration is the degree to which members of a national political system have facilities for communications and transactions with other members who hold similar roles in the stratification system. The relationship between communications and integration is based on the assumption that the extent of interaction between two people correlates positively with their mutual liking and cooperation.[11] Stimulated by Deutsch's various applications of communications theory,[12] studies of integration commonly focus on measures of transactions as indicators of integration, and indicators of travel flows, message flows, and trade flows are regularly employed.[13] But for the purposes of cross-national research, as opposed to international relations research, these measures are very difficult to obtain and to standardize.[14] Therefore, instead of measuring transactions within nations, we suggest measuring horizontal integration by the extent to which facilities for communication, or communications potential, are developed within nations.[15]

Vertical integration is the degree to which political actors in different strata are linked together. In the literature on integration this dimension is commonly described as the mass-elite gap,[16] and it is argued that unless the elite is able rapidly to communicate its commands/decisions to the mass, and able to respond to the demands or feedback from the mass public, the authority of the elite and the cohesion of the political system is likely to decline.[17] The underlying causes of inadequate communication between mass and elite are the value and identity incongruities between these two populations. As Shils suggests,[18] political integration is a function of the inculcation of the core values of the elite population in the minds of the mass public on the periphery of the central decision-making institutions. Shils further argues that the identities of the elite tend to become valued in themselves, and we suggest, therefore, that the mass-elite gap be operationally measured in African nations as the difference between the proportional representation of ethnic groups in the elite and the proportion of these groups in the total population.[19] A different approach is to measure the extent of mobility between mass and elite populations in terms of the access to the skills and rewards characteristic of elite status.[20]

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[9] Karl W. Deutsch *et al.*, *Political Community and the North Atlantic Area* (Princeton: Princeton University Press, 1957), p. 5.

[10] For other useful summaries of dimensions of integration, see W. S. Landecker, "Types of Integration and Their Measurement," *American Journal of Sociology*, 56 (January, 1951), 332–40, and Weiner, "Political Integration and Political Development."

[11] Of course there is the rival hypothesis that increasing interaction increases hostility and conflict unless the interacting parties share similar values relevant to the activity in which they interact. See Lewis A. Coser, *The Functions of Social Conflict* (London: Routledge and Kegan Paul, 1956), p. 62, and Philip Jacob and James V. Toscano, eds., *The Integration of Political Communities*, p. 101.

[12] Karl W. Deutsch, *The Nerves of Government* (New York: The Free Press, 1963), and *Nationalism and Social Communication* (Cambridge, Mass.: MIT Press, 2nd ed., 1966).

[13] Karl W. Deutsch, "Transaction Flows as Indicators of Political Cohesion," in Jacob and Toscano.

[14] At the time this paper was written we were able to obtain information on domestic mail flows for only seven of the African countries, internal telephonic communications measures for even fewer, and internal trade patterns for none at all. A particularly troublesome problem, even if the data were available, would be to determine a method of dividing nations into comparable internal units (regions) in order to compare the spatial distribution of transactions within them.

[15] The operational definitions of communications potential were initially given in terms of 76 individual measures which we conceptually grouped into dimensions of private, commercial and urban communications. Factor analysis of these measures showed a stable structure of four factors which we labelled mass communications, personal communications, commercial communications and aviation communications. The content of the first three of these factors was then analyzed, and high-loading variables within each factor were separately analyzed to produce unidimensional factors, the factor scores for which gave three measures of communications potential. The content of these factors is shown in the appendix to this paper. In addition to these three indices of the scale of communications potential, we also use four static and three dynamic measures which were unique variables in the original factor solution: the percentage of the national population living in cities over 20,000, in 1965; the percentage change in the proportion of the national population living in cities over 20,000, 1955–65; the primacy of the largest city, which is the ratio of the population in the largest city to the population in the second largest city; the number of miles of improved road per 1,000 square miles of land area, c. 1965; the number of miles of railroad track per 1,000 square miles of land area, c. 1965; the percentage change in the number of radios in use, 1960–65, and the percentage change in the number of commercial vehicles, 1958–66. Data and sources for these measures are given in Morrison *et al.*, *Black Africa*, chapters 5 and 15, and Part II. The use of multiple indicators here, and for all the theoretical dimensions of integration outlined in this paper, is a fundamental methodological procedure for controlling for error in nonexperimental data. See Hubert M. Blalock, Jr., "Estimating Measurement Error Using Multiple Indicators and Severa Points in Time," *American Sociological Review*, 35 (February, 1970), 101–111, and Blalock, "Multiple Indicators and the Causal Approach to Measuremen Error," *American Journal of Sociology*, 75 (September, 1969), 264–273. Also see "Introduction," in Morrison *et al.*, *Black Africa*.

[16] See Leonard Binder, "National Integration and Political Development," and Myron Weiner, "Political Integration and Political Development."

[17] It is also argued that the integration of the elite

Value integration is the degree of value congruence in a society.[21] According to the fundamental hypothesis of structural functional theories of the social system, societies are distinguished from fluctuating aggregates of interacting individuals by the boundaries of value consensus to which the members of the system subscribe. Although it is of course difficult to specify the range of common values that should be considered, two dimensions seem important: (1) common values relating to the definition of the political community and to membership in it,[22] and (2) common values relating to the structure of authority and the legitimation of the exercise of power.[23] Value integration in nation-states is hindered by the extent of sociocultural pluralism within them—pluralism in the sense of segmentalized societies, not in Dahl's sense of power differentiation.[24] Pluralism can be measured in terms of language and religious heterogeneity in nations,[25] and in Af-

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itself, rather than mass-elite integration, is of primary importance to national integration. See Ake, *A Theory of Political Integration*.

[18] Edward Shils, "Centre and Periphery," in *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi* (London: Routledge and Kegan Paul, 1961).

[19] The greater, or more significant, the difference between elite and mass populations in terms of ethnic identity and values, the greater the mass-elite gap, and the less integrated the political system. The measure of disproportionality used for this paper is based on a chi-square statistic measuring the difference between the observed ethnic group proportions in the national population, and the proportions of cabinet members belonging to different ethnic groups. The greater the value of the chi-square statistic the greater the disproportionality, and the greater the mass-elite gap. The chi-square value was then used to calculate a C-coefficient, which normalized the chi-square statistic, which is highly sensitive to the number of observations used in its calculation. The measures used in this paper are based on the composition of the cabinet membership at the time of independence and in either 1967 or the year before a coup in those countries where coups have occurred. A third measure is based on the change in the value of the C-coefficients for those two periods of time. The data for ethnic proportionality in national populations and cabinets is given in Morrison *et al.*, *Black Africa*, Part II.

[20] The measures of mass-elite mobility used in this paper relate to the access to advanced education: (a) the number of students enrolled in institutions of higher learning as a percentage of the relevant age group, 1961–62; (b) secondary-school enrollment as a ratio of the relevant age group, 1957; (c) secondary-school enrollment as a ratio of the relevant age group, 1961, and (d) the average annual rate of increase in the secondary-school enrollment ratio, 1957–61. The data for these variables are taken from *The Development of Higher Education in Africa* (UNESCO: Report of the Conference on the Development of Higher Education in Africa, 1962), and *World Survey of Education*, Vol. III, *Secondary Education* (New York: International Documents Service by arrangement with UNESCO, 1961). For the importance of higher education as a definition of elite status in Africa, see P. C. Lloyd, ed., *The New Elites of Tropical Africa* (London: Oxford University Press, 1967).

[21] See Philip E. Jacob, "The Influence of Values in Political Integration," in Jacob and Toscano, eds., *The Integration of Political Communities*, pp. 209–246.

[22] These values structure identity and loyalty in the political system. As Deutsch argues: "The population of different territories might easily profess verbal attachment to the same set of values *without having a sense of political community that leads to political integration.* The kind of sense of community that is relevant for integration . . . turned out to be rather a matter of mutual sympathy and loyalties; of 'we-feeling,' trust and mutual consideration; of partial identification in terms of self-images and interests; of mutually successful predictions of behavior and of cooperative action in accordance with it. . . . Peaceful change could not be assured without this kind of relationship." Karl W. Deutsch *et al.*, *Political Community in the North Atlantic Area*, p. 36.

[23] The importance of congruence in the authority values in different subsystems of society is the basis of the argument in Harry Eckstein, *A Theory of Stable Democracy* (Princeton: Princeton University, 1961).

[24] "A society is pluralistic to the extent that it is structurally segmented and culturally diverse." Pierre van den Berghe, "Towards a Sociology for Africa," *Social Forces*, 43 (October, 1964), 11–18.

[25] Our measures of linguistic pluralism are: a) the number of different languages spoken in each country; b) the percentage of the national population who speak the major vernacular language, and c) a measure of linguistic homogeneity indicating the ratio of speakers of the dominant vernacular to second vernacular speakers, c. 1968. Religious pluralism is measured by: a) the proportion of the population that identifies with Islam, b) the proportion of the population that identifies with Christianity, c) the ratio of the population identifying with the predominant universal religion (Islam or Christianity) to the population identifying with the other major religion, and d) a measure of religious fractionalization calculated as

$$1-\sum_i PR_i^2$$

where PR_i is the proportion of the population identified with Christianity, Islam, Independent Churches, and Traditional religions. The data used in the construction of these variables is given and documented in Morrison *et al.*, *Black Africa*, Chap. 2.

rica particularly, by the cultural differences between ethnic units, or homogeneous value clusters, in national populations.[26]

Centralization is the extent to which the individuals in a political system are subject to the decisions of a central institution, which has the capacity (and the right) to mobilize coercion to enforce compliance with its decisions.[27] Thus the integration of political systems may be thought of as (1) the extent of development of centralized political institutions which increasingly authorize the distribution of values in the society,[28] and (2) the expansion of the coercive facilities available to central political institutions for the enforcement of compliance to their decisions.[29] The institutionalization of politics and the extension of power together constitute the cutting edge of political integration, and minimal integration or cohesion among members of political systems can be enforced by naked, and, indeed, illegitimate power, although we do not expect systems so integrated to maintain themselves in the long run. Operationally, we can assess the centralization of political authority in terms of the homogeneity of party and legislative organization, and in terms of the increasing economic role of government in spending and taxing.[30] The coercive potential available to governments can be measured by the growth of military and security systems and by the coercive restrictions imposed by goverment on people and organizations.[31]

This brief discussion of the conceptual dimensions of integration can be summarized in Table 1, before we go on to discuss the conceptual and operational treatment of political instability.

Instability has already been defined in this paper as a condition in which social conflict is no longer adequately regulated by the institutional and integrative mechanisms of the social system, resulting in a rapid and relatively extreme breakdown in the structure of social relationships.[32]

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[26] We have elsewhere defined and described ethnic units in each of our 32 African nations. See "National Integration and Ethnicity," in Morrison *et al.*, *Black Africa*. Each of the ethnic units has been coded on a set of variables, ordinally scaled and adapted from the codes in George Peter Murdock, *The Ethnographic Atlas* (Pittsburgh: University of Pittsburgh Press, 1967). A summary description of 24 such variables is given in Morrison *et al.*, *op. cit.* The national variance across ethnic units has then been calculated for each of these variables, giving a number of measures of cultural pluralism. See Morrison *et al.*, *Black Africa*, chapter 16. Factor analyzing these measures we obtained a solution showing three major dimensions of cultural pluralism in these nations, and these factors were then separately factor analyzed to give unidimensional factors indicating the national variance in the ethnic units' patterns of ecological adaptation, authority and stratification, and social organization. See the appendix to this paper for the content of these unidimensional factors, the factor scores for which are used as three measures of cultural pluralism in our analysis.

[27] Cf. the definition of the state given by Max Weber in *The Theory of Social and Economic Organization*, (Glencoe, Illinois: The Free Press, 1947).

[28] This has to do with what Deutsch calls the range of integration—"the range of rewards and deprivations for the component units, by which an integrated relationship is maintained among them." Deutsch, *The Analysis of International Relations*, p. 159.

[29] Compare the argument of Etzioni: "A political community is a community that possesses three kinds of integration: (a) it has an effective control over the use of the means of violence . . . (b) it has a center of decision-making that is able to affect significantly the allocation of resources and rewards throughout the community; and (c) it is the dominant focus of political identification for the large majority of politically aware citizens." Etzioni, *Political Unification*, p. 4.

[30] The measures we use are: a) legislative fractionalization at independence, calculated as

$$1 - \sum_i PR_i^2$$

where PR_i is the proportion of legislative seats held by the *i*th of a set of different parties; b) the number of political parties established from the date of a nation's independence to 1969; c) percentage of the wage labour force employed in the public sector; d) rate of growth in the ratio of government spending to GNP, 1963–65; e) rate of growth in government revenue as per cent of GNP, 1963–65; f) rate of growth in central government revenue, 1963–66; and g) central government revenue, c. 1963. The data for these variables are given in Morrison *et al.*, *Black Africa*, chaps. 7, 8, and 9.

[31] A large number of measures relating to military and security forces in African nations were factor analyzed, and from this analysis we constructed three unidimensional factors indicating the size of military and security systems, the budgetary allocations to such systems, and the amount of foreign military aid in these nations. The content of these factors is shown in the appendix to this paper. In addition to the factor scores derived from these three scales, we use three variables that were unique to the original factor solution as measures of coercive potential: the percentage change in the national defence budget as a proportion of GNP, 1964–67; the number of political parties banned through the period independence to 1969, and the number of persons arrested for political offences in the period independence through 1969. For the sources of military data and a presentation of selected indicators, see Morrison *et al.*, *Black Africa*, chap. 12. The data on parties banned is taken from Part II of that book, and the data on political arrests have been coded from the sources used in the collection of data on political instability reported in Chapter 11.

[32] This conceptualization of political instability, and the theory presented here, should be sharply distinguished from more common approaches to the analysis of political instability, which are based on a conceptualization of political instability as aggression directed by individuals against the political system and which explain such aggression by variation in indices of frustration, or relative deprivation, such as the ratio of social want satisfaction to social want formation. See Ivo K. and Rosalind L. Feierabend "Aggressive Behavior Within Politics 1948–1962: A

Table 1. Dimensions of Integration and Operationalizations: Summary

INTEGRATION

HORIZONTAL INTEGRATION	VERTICAL INTEGRATION	VALUE INTEGRATION	CENTRALIZATION	
Communications Potential	Mass-Elite Gap	Pluralism	Centralization of Authority	Coercive Potential
(1) Mass communication (factor score) (2) Personal communication (factor score) (3) Commercial communication (factor score) (4) % pop. in cities over 20,000, c. 1965 (5) Urban primacy, c. 1966 (6) Improved roads per 1,000 sq. miles, c. 1965 (7) Railroad track per 1,000 sq. miles, c. 1965 (8) % change in radios, 1960–1965 (9) % change in commercial vehicles, 1958–1966 (10) % change in proportion of pop. in cities over 20,000, 1955–1965	(11) Elite representation (C), independence (12) Elite representation (C), 1967 or pre-coup (13) Change in elite rep., (12) minus (11) (14) Enrollment in higher education as % of relevant age group, c. 1961 (15) Enrollment in secondary school as % of relevant age group, c. 1957 (16) Enrollment in secondary school as % of relevant age group, 1961 (17) Average annual rate of increase in secondary enrollment, 1957–1961	(18) Variance in ecological adaptation (factor score) (19) Variance in stratification/authority (factor score) (20) Variance in social organization (factor score) (21) % speakers of major vernacular language, c. 1968 (22) Number of languages spoken, c. 1968 (23) Ratio of speakers of dominant vernacular to speakers of second language, c. 1968 (24) % identifiers with Islam, 1966 (25) % identifiers with Christianity, 1966 (26) Religious fractionalization, 1966 (27) Ratio of % dominant universalistic religion to % secondary religion, 1966	(28) Legislative fractionalization at independence (29) Number of political parties established between independence and 1969 (30) % of wage and salary workers employed in the public sector, c. 1962 (31) Central govt. revenue, c. 1963 (32) Govt. revenue as % GNP, c. 1963 (33) Direct taxes as % total govt. revenue, c. 1963 (34) Rate of growth govt. revenue as % GNP, 1963–1965 (35) Rate of growth central govt. revenue, 1963–1966 (36) Rate of growth govt. spending as % GNP, 1963–65	(37) Domestic coercive potential (factor score), c. 1966 (38) Military spending (factor score), c. 1966 (39) Foreign military aid (factor score), c. 1966 (40) Number of parties banned, independence to, 1969 (41) Number of arrests for political offense, independence to, 1967 (42) % change of defense budget as % of GNP, 1963–67

Cross-National Study," *Journal of Conflict Resolution*, 10 (September, 1966); James Davies, "Toward a Theory of Revolution," *American Sociological Review*, 37 (1962); Raymond Tanter and Manus Midlarsky, "A Theory of Revolution"; D. P. Bwy, "Political Instability in Latin America . . ." and Ted Gurr, "A Causal Model of Civil Strife . . ." The theoretical basis for these approaches is frustration-aggression theory, which is well summarized in Leonard Berkowitz, *Aggression: A Social Psychological Analysis* (New York: McGraw-Hill, 1962), and reviewed by Ted Gurr in "Psychological Factors in Civil Violence," *World Politics*, 20 (January, 1968), 245–278, and *Why Men Rebel*, Chaps. 2 and 3. Our approach should also be contrasted with that in recent research based on minimal theoretical focus, in which the data have been allowed to "speak for themselves" providing factor solutions of indicators of conflict behaviour, which are then labeled as types of political instability—e.g., turmoil and anomic violence. See, for example, Rudolph J. Rummell, "Dimensions of Conflict Behavior Within Nations, 1946–59, *Journal of Conflict Resolution*, 10 (March, 1966), 65–73, and Raymond Tanter, "Dimensions of Conflict Behavior Within and Between Nations 1958–60" *JCR*, 10 (March, 1966), 41–64. For details of our own conceptualization of political instability, and a review of related work, see Morrison and Stevenson "Political Instability in Independent Black Africa: More Dimensions of Conflict Behavior Within Nations," *The Journal of Conflict Resolution*, 15 (September, 1972), 347–368.

Political instability is the special case of breakdown in the structure of political authority. In the contemporary political science literature, common language descriptions of aspects of political instability are the only comparable concepts used in analysis of this subject. No matter whether the political scientist studies political violence,[33] political instability,[34] conflict behavior[35] or civil strife,[36] the phenomena under investigation include riots, civil wars, rebellions, coups d'état and revolutions. But aside from the translation of ordinary terminology into operationalized variables, the theoretical categorization of the phenomena of political instability is varied, and it depends more heavily on statistical explanation than on analytic needs.[37]

Our own theoretical orientation holds that instability results from conflict; we define instability in terms of the social organization of the conflicting parties, and the intensity of the violence they utilize to alter their authority relationships.[38] By these criteria, three conceptually distinct types of political instability can be isolated: elite instability, communal instability, and mass instability.

Elite instability is the unregulated removal of persons from the command positions they hold in political institutions. This removal may range from cabinet turnover (not as a result of elections) to the wholesale replacement of a set of decision makers by an alternate elite in a coup d'état. Elite instability is characterized by a low intensity of violence, and by a relative stability in relations of authority. Only a small number of people is directly involved in the "action," and there is no major reorganization or redistribution of power and the ruling class. Nevertheless, although there is some argument on this score in the literature,[39] we regard elite instability, and the coup in particular, as a form of political instability. In the postindependence history of African nations, up until the end of 1969, there have been some 24 successful coups d'état in Burundi, Central African Republic, Congo (Brazzaville), Zaire, Dahomey, Ghana, Mali, Nigeria, Sierra Leone, Somali Republic, Sudan, Togo, Uganda, and Upper Volta. In the same period there have been some 11 attempted coups, 74 plots, and 17 mutinies in the 32 African nations we are concerned with here.[40]

Communal instability is violent conflict between subsystems (e.g., ethnic or regional communities) which is aimed at a radical restructuring of the authority relationships between the national or central government and the subunits. Communal instability is characterized by high intensities of violence, and by fairly marked changes in the structure of authority. Communal instability includes rebellions, civil wars, irredentism, and ethnic violence of other kinds. Rebellious communities aim to establish increased political autonomy for themselves; communal groups involved in civil war aim to secede from the existing sovereign system, or to take it over; and irredentist movements aim to align themselves with political systems in which they have closer communal ties with citizens and political authorities. In postcolonial Africa, civil war has been a painful shock to the political development of Burundi, Chad, Zaire, Ethiopia, Ivory Coast, Nigeria, Rwanda, and Sudan. There have been rebellions in Chad, Congo (Brazzaville), Ghana, Kenya,

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[33] Ted Gurr, "Psychological Factors in Civil Violence."

[34] Ivo K. Feierabend, Rosalind L. Feierabend, and Betty A. Nesvold, "Social Change and Political Violence: Cross-National Patterns," in *The History of Violence in America,* Davies and Gurr, eds., pp. 632–687.

[35] Rummell, "Dimensions of Conflict Behavior Within Nations, 1946–59."

[36] Ted Gurr, "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review,* 67 (December, 1968) 1104–1124.

[37] See the discussion in Harry Eckstein, "On the Etiology of Internal War," *History and Theory,* 4 (1965), especially p. 135. Factor analysis has been widely used as an inductive approach to typologies of instability. Factor solutions of data on political instability are variously entitled revolution, subversion, and turmoil (Rummell "Dimensions . . . 46–59"); internal war and turmoil (Tanter, "Dimensions . . . 58–60"); or organized and anomic violence (Bwy, "Political Instability in Latin America"). Such has been the impact of these empirical findings that direct coding is now organized in terms of a factor-based typology of internal war, conspiracy, and anomic violence (Gurr, "A Causal Model of Civil Strife"). Ironically, the complexities of these typologies are for the most part ignored in analysis of the conditions giving rise to political instability, in which the dependent variable is most often an aggregate measure of civil strife or instability.

[38] By the intensity of violence we mean the number of persons involved in unpredictable or physically destructive behavior; the duration of such behavior; and the extent of the alterations in the established political structure brought about by such behavior.

[39] A review of the relevant writings on the subject is given by David C. Rapoport, "Coup d'Etat: The View of the Men Firing Pistols," in *Nomos VIII: Revolution,* Carl J. Friedrich, ed. (New York: Atherton Press, 1966).

[40] These data, and all measures of political instability used in this paper, are summarized in chapter 11, "Political Instability," in Morrison *et al., Black Africa.* Part II of that book contains abbreviated verbal descriptions of the events coded. The sources used for the collection of these data are also detailed there.

Mali, Nigeria, Somali Republic, Sudan, Uganda, and Zambia; and irredentism has intensified domestic and international problems in Ethiopia, Ghana, Kenya, and Uganda.

Mass instability involves revolts or revolutions in which agents and supporters of the established political authority are attacked by insurgents whose primary source of common identity is their associational membership in a political movement, rather than any ascriptive communal identity. Such mass instability is very difficult to isolate in contemporary African nations.[41] The revolt in Zaire coordinated by the *Comité Nationale de Liberation,* the revolt in Malawi in support of Chipembere, the sporadic revolt in Niger led by the Sawaba party, and the revolt against the Sudanese military government in 1964 are the only instances of significant political violence in which associational rather than communal organization may have been a predominant characteristic of the insurgents in the period discussed here.

In this paper, we are concerned with the incidence of elite and communal instability in Africa, and the dependent variables in the statistical analysis are weighted indices constructed from the number of events coded as coups d'état, attempted coups, plots, civil wars, rebellions, irredentism, and ethnic violence in African nations from their dates of independence to the end of 1969.[42]

A Model of National Integration and Instability

Since concepts and their operationalizations do not in themselves constitute theory, this section is designed to formulate a testable model of the theoretical relationships among the different processes of integration and instability we have conceptualized.[43] This model can be best analyzed with reference to Figure 1. Figure 1 does no more than link diagrammatically the conceptual dimensions of integration and instability that we have outlined so far. What is required in this section is that the theoretical relationships between these dimensions be articulated in the form of empirically testable hypotheses. The basic proposition of this model may be stated as follows:

> The greater the communication between politically relevant units in the political system, the less likely will instability characterize that system.

As indicated earlier, this proposition owes much to the work of Deutsch, and it implies the importance of close communication between elites and masses and among different communal groupings in the population. As indicated in Figure 1, the relationship between communications potential and instability is mediated by the intervening effects of centralization of authority and coercive potential: that is, the direct relationship between communications potential and either elite or communal instability is critically modified by the indirect effects of centralization of authority and coercive potential. These hypotheses may be formally articulated as follows:

H1.1 The *direct relationship between communications potential and elite instability* is

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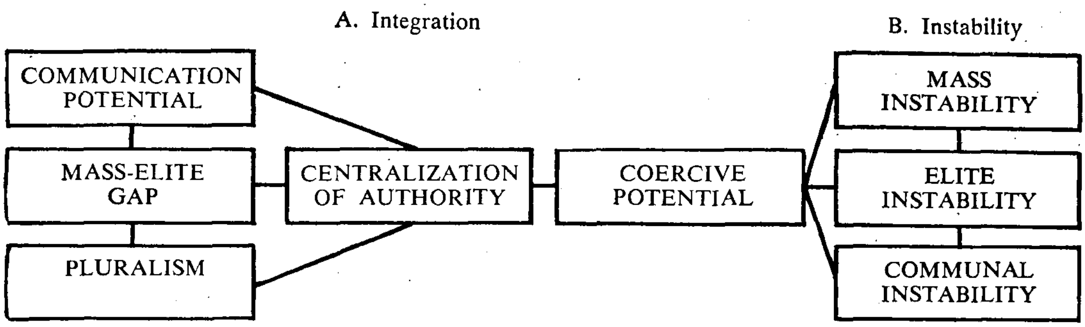
[41] Since we are dealing with the postcolonial history of Sub-Saharan Africa, we are not concerned with anticolonial revolts or with the liberation struggles in the Portuguese territories and white-controlled Southern Africa.

[42] *Elite instability* is measured as follows: Successful coups d'état are given the numerical weight 5; attempted but unsuccessful coups are weighted 3; and plots, 1. The index for each country is the sum of all such events so weighted in the period from independence to 1970. Communal instability is measured as follows: Civil wars are given the weight 4; rebellions, 3; irredentism, 2; and ethnic violence, 1. The index for each country is calculated by summing the numerical weights for each event multiplied by the number of years in which such an event was reported. It should be noted that the individual measures used in the calculation of these indices are highly intercorrelated, and that they represent independent dimensions in factor analytic results based on a much larger set of variables. Those results indicate support for our theoretical distinctions between elite, communal, and mass instability, and they also indicate additional evidence for the existence of an independent dimension measuring "turmoil," which is based on indicators of strikes, riots, demonstrations, numbers killed and arrested, and terrorism. Although this dimension has been regularly reported in other studies, we do not use it as a dependent variable in this study, because we believe that the events involved are the least reliably reported and can only very loosely be interpreted as attempts to change authority relationships in the political system. See Morrison and Stevenson, "Political Instability in Independent Black Africa . . ."

[43] The notion of process as applied to the discussion so far may be confusing, but the distinction between static and dynamic models of integration is in part artificial. We see process as continuous interrelationships among characteristics of a system over time—i.e., as a series of state descriptions over time. What is important about the distinction is that it sensitizes the researcher to the consideration of historical, or time-series, relationships. Although we are not investigating the major questions of what dimensions of integration precede others in points of time in the development process, and although we cannot in this paper explore the many possibilities of lagged relationships between aspects of integration and instability, we do intend to explore the differences that result from testing our model of integration and instability with static, cross-sectional data, as opposed to tests based on variables measuring rates of change over time. Our expectation is that the results will be quite different, with important implications for the analysis of integration and instability.

Figure 1. Components of a Model of National Integration and Instability



inverse—i.e., the greater the level of communications potential, the less the likelihood of elite instability.

H1.2 The direct relationship between communications potential and communal instability is inverse—i.e., the greater the level of communications potential, the less the likelihood of communal instability.

H1.3 The direct relationship between communications potential and either elite or communal instability is negative, *and* the intervening effects of centralization of authority and coercive potential operate in the same direction—i.e., increasing centralization of authority and coercive potential further decreases the probability that either form of instability will result from high levels of communications potential.

H1.4 The strength of the direct relationship between communications potential and instability is critically modified by the intervening effects of centralization of authority and coercive potential—i.e., the direct relationship will tend toward zero, when the intervening effects are taken into account.

These basic hypotheses are explained by the assumption that increased communications between actors decreases the likelihood of conflict between them, and leads to mutual sentiments of liking; in the case of relations between government and public, increased communications leads to legitimacy on the one hand and responsiveness on the other. We cannot validly operationalize legitimacy in terms of aggregate measures available to us, and therefore the fundamental relationship between communications, coercive potential, centralization of authority, and legitimacy must be assumed. In these circumstances, there are rival hypotheses that must be clearly articulated. If increasing interaction and communication within the political system does not produce the hypothesized legitimacy, but rather increased conflict between the governmental elite and other population groups, then the likelihood of instability is increased by increasing communication between these units.

The assumed relationship between instability and legitimacy is so critical that it may be useful to spell it out. Even though it cannot be tested in this paper, we will have recourse to the following hypothesis as an explanation of either the testing or rival hypotheses stated elsewhere:

> The greater the legitimacy of the established political system, the less the likelihood of political instability.

This hypothesis is part of the common wisdom of political sociology, and is in fact almost definitional. As Lipset defines it, legitimacy is "the capacity of the system to engender and maintain the belief that the existing political institutions are the most appropriate ones for the society."[44] Low legitimacy therefore implies the belief on the part of a significant section of the population that the existing political institutions are not appropriate, and the suggestion is very strong that more appropriate institutions will be introduced. The proposition is not tautologous, however, since an illegitimate government is still able to enforce subjection, and since belief that the established institutions are inappropriate does not necessarily imply violent action on the part of those who hold that belief. The response of the political authorities to different levels of legitimacy may be stated in the following hypothetical form:

> The greater the legitimacy of the established political system, the more will centralization of authority be facilitated.
>
> The less the legitimacy of the established political system, the more coercive potential will be required by political authorities to avoid instability, and therefore the more likely will the level

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[44] Seymour Martin Lipset, *Political Man* (New York: Anchor Books, 1963), p. 64.

of coercive potential at the disposal of the government increase.

The intervening effects of increasing centralization of authority and coercive potential are also contingent on the level of legitimacy generated by the existing governmental process. Ever since Machiavelli so tersely advised his prince either to repress his subjects thoroughly (preferably with the help of mercenaries with no affiliation with the people), or else to ensure that they liked him, it has been clear that the right of might will do in the short run, but that political regimes require legitimation from their subjects for long-range stability. Unfortunately, it is exceedingly difficult to determine what balance of might and legitimacy is necessary for stability, and it is equally difficult to obtain aggregate data on the hearts and minds of peoples, although relatively easy to gain information on the means of coercing them. Without adequate measures of legitimacy, it is difficult to determine the effects of either the centralization of authority, or increasing coercive potential on the stability of the political system, but the hypothetical relationships may be stated as follows:

H2.1 The greater the centralization of authority, the less likely is political instability of any kind.

H2.2 The greater the level of coercive potential available to the government, the less likely is political instability of any kind.

Hypothesis 2.1 is contingent on the presence of coercive potential available to the government sufficient to operate effectively in the geographical periphery of the political system, and also on the presence of adequate means of communication. This is true for any but the most microcosmic of states, as Deutsch argues when he points out the inability of political control mechanisms to operate beyond a given range in the absence of efficient communication and feedback mechanisms.[45]

Hypothesis 2.2 is too simple a statement of the relationship of coercive potential to instability. Aside from the contingent effects of levels of legitimacy, there are effects of time and of the application of coercion that have to be considered. Etzioni hypothesizes that the relation between the application of coercive power and instability is curvilinear;[46] Gurr has adduced evidence for such a relationship from a comparison of 114 nations, and Bwy has provided support for this hypothesis in his study of conflict in Latin America.[47] Guetzkow has explored the effect of negative and positive reinforcement of loyalty—a key consideration in this context.[48] In addition, the relationship between stability in new nations and the degree of foreign military assistance they receive needs careful exploration. Recent analysis of the impact of foreign aid on the stability of new nations seems theoretically weak, and the results far from clear, since no attempt is made to distinguish between military and other aid.[49]

At this stage, we can discuss the implications of the mass-elite gap and the degree of pluralism as dimensions of integration relating to political instability. We expect that these two variables have differential effects in the models of elite and communal instability, which may be hypothesized as follows:

H3.1 The direct relationship between the level of the mass-elite gap and elite instability, or communal instability, is positive, *but* the relationship between the mass-elite gap and elite instability is stronger than that between the mass-elite gap and communal instability.

This hypothesis is based on the theoretical assumption that elite instability is a result of inter-elite conflict, in which potential political elites find it possible to articulate mass grievances resulting from a lack of significant representation, or at least expect to legitimize their coups d'état by promises of increasing the representativeness of government. Communal instability, although affected by the loss of legitimacy resulting from unrepresentative government and by small and restricted elite strata, is less a result of conflict between elites, or between elites and the mass public, than it is a result of conflict arising between communal groups holding incompatible values. This suggests the following hypothesis:

H3.2 The direct relationship between the degree of pluralism and communal instability, or elite instability, is positive, *but* the relationship between the degree of pluralism and communal instability is stronger than that between pluralism and elite instability.

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[45] See Deutsch, *Nationalism and Social Communication.*

[46] Etzioni, *Political Unification,* p. 94.

[47] Gurr, "A Causal Model of Civil Strife," 1116, and Bwy, "Political Instability in Latin America," 49–50.

[48] Harold Guetzkow, *Multiple Loyalties: Theoretical Approach to a Problem in International Organization* (Princeton: Center for Research on World Political Institutions, Princeton University, 1955), p. 33. Guetzkow argues for the greater effectiveness of positively reinforced rather than punitively induced loyalty to political systems.

[49] See Midlarsky and Tanter, "Toward a Theory of Political Instability in Latin America."

As with the relationship between communications potential and instability, we hypothesize that the direct relationships between the level of the mass-elite gap, or the degree of pluralism, and instability are critically modified by the inhibiting effects of the centralization of authority and the degree of coercive potential. Formally stated:

H3.3 The direct relationship between the level of the mass-elite gap and either elite or communal instability is positive, *but* the intervening effects of the centralization of authority and coercive potential operate in the opposite direction: i.e., as centralization of authority and coercive potential *increase*, the probability that instability will result from the mass-elite gap *decreases*.

H3.4 The direct relationship between the degree of pluralism and either elite or communal instability is positive, *but* the intervening effects of the centralization of authority and coercive potential operate in the opposite direction: i.e., as centralization of authority and coercive potential *increase*, the probability that instability will result from pluralism *decreases*.

H3.5 The strength of the direct relationships between the mass-elite gap, pluralism and instability are critically modified by the intervening effects of centralization and coercive potential—i.e., these relationships will tend towards zero, when the intervening effects are taken into account.

Other relationships that might be discussed include the interactive effects of increasing communications potential on the relationships between the mass-elite gap, pluralism, and political instability. Like centralization and coercive potential, increasing communications potential is hypothesized to decrease the probability that political instability will result from high levels of the mass-elite gap or pluralism. In this paper however we shall consider only the intervening effects of centralization and coercion.

Before proceeding to the tests of the propositions put forward here, we should make three points: First, we do not expect that the empirical results of the hypothesized relationships between concepts will be identical no matter what the operationalized variable used. In terms of the multitrait logic discussed in this paper, the converging evidence for these hypotheses will increase as the number of operationalizations of the concept that confirm the hypothesis increases. Nevertheless, we expect variation in the relations between the same concepts differently operationalized, and it will be a contribution in itself to explore the ambiguities which imprecisely operationalized theories pose. Second, we expect the tests of this theory of integration and instability to discriminate between elite instability and communal instability by showing that different processes of integration are more important explanations of each of these dependent variables. Third, we do not expect the explanatory power of either the elite or the communal instability model to be by any means exhaustive. As indicated earlier in this paper, these models deal with only one half of a theoretical framework, the whole of which is necessary to an explanation of political instability.

Methodology

Three features distinguish the methodology used to evaluate the theory of integration and instability presented in this paper: (1) a method of operationalizing concepts, and assessing the reliability and validity of operationalizations, in terms of a multimethod, multitrait analysis of individual indicators; (2) an emphasis on assessing the validity of the variables alleged to measure integration in terms of their converging patterns of intercorrelation with measures of instability; and (3) a causal analysis of the interactive effects of different dimensions of integration on the dependent variables—an analysis which is again based on converging evidence for the significance and directionality of causal linkages in our model, using different combinations of individual indicators. Underlying all three of these approaches is a fundamental stress on the primacy of theory. No data are useful without theoretical criteria for their relevance. No individual indicator is in itself either completely reliable or completely valid as an operationalization of a theoretical concept—the validity of indicators is finally assessed in terms of theoretical assumptions about the correspondence of a measure and a concept, and about the covariation between measures of different concepts. And no statistical measures are free from the threats of chance or spurious covariance in two or more indicators, so that it is ultimately theoretical explanations on which decisions about the significance of statistical measures depends.

Reliability and Validity. One does not directly observe nations, but the empirical comparison of social processes that take place in nations depends on information about the characteristics of entities (individuals, groups, organizations and artifacts) and events which can be directly observed. National characteristics are then aggregate descriptions of the quantity, central tendency, or distribution of different characteristics of any entities or events of the same kind that are observed in a nation. The critical problems of cross-national research are (a) whether entities in one nation are functionally equivalent to entities in another, and therefore whether their characteristics may be compared; (b) whether the complications involved in combining observations of micro-entities make for so great a degree of error that the aggregate description of the macro-entity is fallacious; and (c) whether conceptual categories developed for the description of social action can sensibly be applied to the analysis of nations which do not themselves act.

We cannot go into these problems here[50] and must confine ourselves to the assumption that all measures of the characteristics of actors and social action involve a margin of error, no matter what entities are considered, and no matter how great the complexity of index construction or aggregation in the measures. Given the ubiquity of measurement error, the methodological problem is how to minimize the error term in any sociological equation, by maximizing the correspondence between measures of the same indicator taken by different observers of the same conceptual phenomenon, and by maximizing the aggregation of the "true scores" given by different indicators of the same conceptual phenomenon. That the two questions are related is indicated in the following definition of the terms reliability and validity.

> Reliability is the agreement between two efforts to measure the same trait through maximally similar methods. Validity is represented in the agreement between two attempts to measure the same trait through maximally different methods.[51]

In these terms, we have used multiple indicators to operationalize all of the conceptual categories in our theory of integration and instability. When possible we have used measures taken by different observers of the same indicator, and we have consistently taken measures derived from different methods of observation —either measures of different characteristics, or measures aggregating the characteristics of different entities.[52] Two general procedures are then used to increase the validity and reliability of our operationalizations. In the first place, we have developed through factor analysis unidimensional indices as measures of our theoretical concepts. First, a factor analysis on the indicators that are felt relevant to the theoretical concept is performed. Then we assess the tenability of the groupings. If the factors are coherent, as in all cases they have proved to be, we then separately factor analyze the indicators attributed to each dimension. This procedure is derived from the perspective of psychometrics which requires that an index be unidimensional. The resulting single factor then generally accounts for 80 per cent or more of the variance in the correlation matrix. Indices based on these factor solutions exhibit the following properties; (1) The expected error in the index should be reduced relative to the error in the component indicators. Since the method of collection and the agencies that collect the data are generally different from item to item in the index, the original measurement errors should be uncorrelated, and the expected error will be reduced in the factor score. (2) The index is based on several variables necessary to cover the single dimension posited (for example, communication may take place via several media types). (3) Variables *not* relevant to the factor do not enter into the calculation of the index for each dimension.[53] The development of unidimensional indices is further illustrated in the appendix to this paper.

A second procedure for assessing the validity and reliability of our measures is illustrated in the next section, in which we examine the correlation between individual measures of the same concept with the dependent variables measuring political instability. If we find converging patterns of similarity in the strength and directionality of the correlations between multiple indicators of a single concept and each dependent variable, we will have additional confidence in our assumption that we are measuring differently, but reasonably accurately, phenomena of the same theoretical kind.

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[50] See the introduction to Morrison *et al., Black Africa,* for a more detailed discussion.

[51] Donald T. Campbell and Donald W. Fiske, "Convergent and Discriminant Validation by the Multitrait-Multimethod Matrix," *Psychological Bulletin,* 56 (March, 1959), 81–105; citation is from p. 83. It should be noted that our own use of the multitrait, multimethod logic is not strictly tied to the argument and method illustrated in this seminal publication. Where Campbell and Fiske are concerned primarily with experimental research, our own work is based on nonexperimental data. Multitrait operationalization in our work, therefore, cannot approximate the standards of strict replication of experimental methods of data collection as a basis for assessing reliability, and the different methods of data collection we use are not strictly comparable to the alterations of treatment and experimental design discussed by Campbell and Fiske. We are also interested in an elaboration of the multimethod framework to include a multi-unit dimension for nonexperimental research: that is, we feel that data measured on different entities, or units of analysis (such as individuals, ethnic groups, and elite groups), should be compared for converging validation of conceptual variables.

[52] In particular we utilize data collected on ethnic groups and members of cabinets, in addition to the more general kinds of statistics aggregating the characteristics of individuals or goods in a nation.

[53] When these procedures are not adopted, factor scores often include the effects of variables which individually load low on the factor, but may be an important influence on the total factor score.

Causal Analysis. Correlational analysis provides only partial validation of a theory. Simple and multiple regression analysis will demonstrate (a) whether different indicators of the same independent variable are valid in the sense that they predict to a dependent variable in a uniform fashion; (b) whether there is converging evidence for the hypothesized direction of association between measures of different concepts, and (c) whether variance in the dependent variable is adequately accounted for by individual indicators of the independent variables or by a set of such indicators in combination. But the criterion of "percentage of variance explained" is inadequate for validating theories about multidimensional social action. We need to know what explains what in a sequence or process of interaction—whether the hypotheses about the interactive effects of intervening variables in the relationships between different dimensions of integration and instability are supported by converging evidence from numbers of tests based on multiple operationalizations of the related concepts.

In order to test the hypotheses in our models of elite and communal instability, we utilize the technique of path analysis,[54] which breaks down bivariate relationships into (1) the direct effect of an independent variable on the dependent variable, and (2) the indirect effects of all other intervening variables on the relationship between that independent variable and the dependent variable. The particular variation of path analysis that we have used is path regression analysis, which calculates the standardized partial regression coefficients describing the relationship between any two variables in our model, controlling for their covariance with all other variables in the model.

The model we are interested in describes the intervening, or indirect effects, of centralization and coercive potential on the relationships between communications potential, the mass-elite gap, or pluralism on the one hand, and both elite and communal instability on the other. For both elite and communal instability twelve paths can be postulated to describe the relevant causal linkages, as illustrated in Figure 2. In this figure, each of the conceptual dimensions is numbered. The direct line linking any two dimensions is called the path between those two dimensions, and is represented in a notational form in which P_{61}, for example, is the path to the dimension numbered 6 (political instability) from the dimension numbered 1 (communications potential). The first subscript after P always represents the dependent variable in a relationship; the second represents the independent variable.

To summarize, the calculation of the path coefficients describing the direct effects of one variable on another is, as we have already noted, a statistical method of breaking down the size of the correlation coefficient or regression coefficient into effects attributable to the unique relationship between an independent and a dependent variable, and effects attributable to the intervening relationships of one or more variables in the relationship between the independent and dependent variables. The arrows in Figure 2 schematically represent the direct and indirect effects linking any number of dimensions in our model. Hence, the path P_{61}, represents the direct effects of communications potential on instability, and the paths P_{65} and P_{51} represent *one* of the indirect paths whereby the effects of communications potential are altered by the level of coercive potential. These interactive relationships, or indirect effects, are described sometimes as joint effects, and are summarized as the product of the path coefficients between the set of variables being considered.

The model of integration and instability diagramed in Figure 2 is a simplified representation of the possible causal linkages between the dimensions of integration and instability. This model is completely recursive with no feedback loops between variables, and the causal relationships between communications potential, the mass-elite gap, and pluralism are not considered. The double-headed arrows indicate

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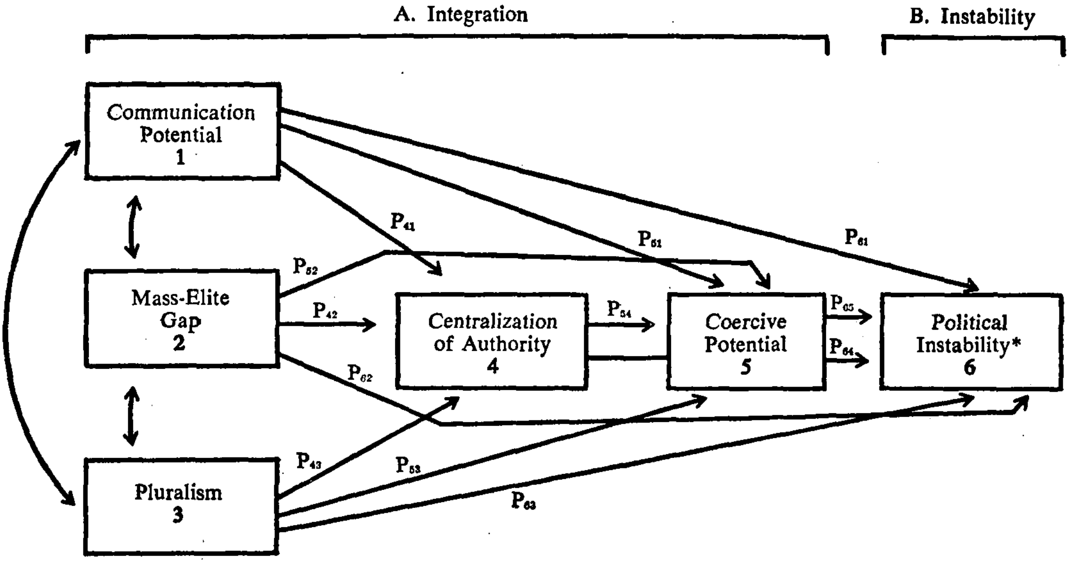
[54] See Sewall Wright, "The Method of Path Coefficients," *Annals of Mathematical Statistics*, 5 (1934), 161–215; and Wright "Path Coefficients and Path Regressions: Alternate or Complimentary Concepts," *Biometrics*, 16 (June, 1960), 189–202; Otis Dudley Duncan, Jr., "Path Analysis: Sociological Examples," *American Journal of Sociology*, 72 (July, 1966), 1–16; Kenneth Land, "Principles of Path Analysis," in *Sociological Methodology 1969* (San Francisco: Jossey-Boss, 1969); Richard Boyle, "Path Analysis and Ordinal Data," *American Journal of Sociology*, 75 (January, 1970) 461–480. A computer program for calculating path coefficients, standardized partial regression coefficients, and the Z-scores of the regression coefficients has been written by Louis Goodman, of the Sociology Department at Yale University, and Donald Morrison. This program was used for computing the results analyzed in this paper.

Figure 2. Integration and Political Instability



* Political Instability = either elite or communal instability.

that we are concerned only with measures of association (correlation coefficients) between these three variables. The analysis of this simplified model would be uncomplicated if we had single indicators of each of the five dimensions of integration that we could be confident were reliable and valid. In that case, the calculation of the standardized regression coefficients for the twelve paths in the models of communal and elite instability would be based on a single solution for a set of simultaneous equations in which instability is a function of communications potential, the mass-elite gap, pluralism, centralization, and coercive potential, in which coercive potential is a function of the first four variables, and centralization a function of the first three. If, however, we were to use all the measures of integration suggested in Table 1, we would have 33,600 possible solutions to the set of equations describing elite or communal instability, in which the values of the path regression coefficients would be based on unique combinations of the operationalizations of integration. In order to reduce this unmanageable complexity, we will select from our analysis of the bivariate correlations three representative indicators of each of the dimensions of integration. This will give 243 possible sets of paths describing the relationship between integration, and a measure of either elite of communal instability, and 81 possible solutions to the path equations relevant to each indicator of integration. Our results, therefore, will be summarized in terms of the percentage of 81 solutions of the model of elite and communal instability in which a specific indicator of integration either directly or indirectly affects the dependent variable. Converging evidence for the strength and directionality of the coefficients describing each path in the models of elite and communal instability will then be used to evaluate our hypotheses.

This approach, we believe, will eventually lead to less emphasis on the primary use of significance tests and multiple R's and *more* emphasis on the construction of methods which rigorously investigate the possibilities of spurious intercorrelation; it will thus generate confidence in explanations by subjecting theoretically derived hypotheses to convergent and discriminant tests. Theoretical coherence and durability in the face of tests based on different samples, different time points, different units of analysis and different measures are the basis of scientific tests of validity, and statistical ingenuity must be the servant of theory rather than its master.

Results

In order to determine how well integration predicts to instability, we shall look first at bivariate correlations between our indicators of different integration concepts to see whether or not they reveal converging patterns of association with the dependent variables. Thereafter, we use a multivariate technique to investigate the predictive power of different integration variables in combination, and to detail those variables that best explain the two kinds of political instability. The relationship between different indicators of integration and elite and communal instability are given in Table 2.

The bivariate correlations indicate that, except for a few individual indicators, the zero-order measures of association between integration and political instability are low, and that they are in general weaker for elite instability than for communal instability. There are, however, certain predictive patterns suggested in this table. (1) There is converging evidence that the scale of mass, personal, and commercial communications potential is positively associated with both forms of instability and quite strongly associated with communal instability. (2) There is converging evidence that dynamic measures of change in communications potential are negatively correlated with both forms of instability, though more strongly with communal instability. (3) There is converging evidence, despite relatively low individual correlations, that static measures of the mass-elite gap are inversely correlated with political instability: i.e., the greater the mass-elite gap (the greater the ethnic imbalance in the elite, or the lower the enrollment in secondary school), the less probable either form of instability. (4) In contrast to (3), there is evidence from both dynamic indicators of the size of the mass-elite gap that the greater the increase in the mass-elite gap, the greater the probability of both forms of instability, though the individual measures of association are low. (5) There is converging evidence from our measures of pluralism that increasing elite, and particularly communal instability, are associated with higher degrees of pluralism. (6) The evidence for the relationship between centralization and political instability is not as coherent as that for other patterns of integration and instability. There is evidence that centralization in the form of homogeneity in party and legislative organization reduces the likelihood of political instability. Centralization in the form of increasing government spending or taxing, however, increases the likelihood of communal instability, but has no consistent relationship with elite instability. Greater control of the economy by central governments, as measured by the percentage of the wage labor force in public sector employment, also increases the likelihood of both forms of instability—elite instability, in particular. (7) There is, finally, converging evidence that higher levels of coercive potential are associated with higher levels of both elite and communal instability, although the dynamic indicator of change in defense spending shows a weaker association than any of the static measures of coercive potential.

From this cursory examination of Table 2, we conclude that there are internally coherent and converging patterns of association with instability within most of the sets of indicators we have selected as measures of different conceptual dimensions of integration. That the individual indicators are seldom strongly associated with the dependent variables is not surprising since we have stressed the multidimensional character of integration. It is reasonable, therefore, to expect that the theoretical explanation of instability in terms of variation in integration will be strongly supported only in multivariate statistical models. The substantial additive power derived by using more than one measure of integration in predicting political instability is shown in Table 3, in which the results of stepwise, multiple regression analysis are reported for elite and communal instability, using all our indicators of integration.

The results reported in Table 3 illustrate that variation in six measures of integration accounts for 61 per cent of the variance in elite instability and 78 per cent of the variance in communal instability for our sample of 32 African states.[55] This finding is surely evidence that neither form of instability can be treated as a random experience in national political development,[56] and that variation in patterns of integration is an important concomitant of patterns of political instability. Table 3 also gives

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[55] These results are corroborated by the results of a stepwise multiple discriminant analysis in which our indicators of integration are used as independent variables, but nominal dependent variables are used to indicate whether or not a country has experienced a coup d'état or civil war, rebellion, and irredentism. This technique calculates a discriminant function, a linear combination of the independent variables, which maximizes differentiation among the groups of cases typologized according to the dependent variables. Each nation used in this analysis is then assigned a score derived from the discriminant function, and that score indicates whether the nation should be classified in one or another of the dependent variable groups. Using this technique, we are able to classify correctly all African nations as having experience or no experience of communal instability, and more than 80 per cent of African nations as having or not having experienced coups d'état, on the basis only of knowledge of their values on indicators of integration. For a discussion of this technique, see Donald G. Morrison and Richard Art, Jr., *A Fortran Program for Stepwise Multiple Discriminant Analysis*, April 1967, Vogelback Computing Center, Northwestern University, and *Behavioral Science*, 13 (January, 1968), 82–83.

[56] Compare the argument in Zolberg, ". . . Political Conflict in the New States of Tropical Africa."

Table 2. Pearson Product-Moment Correlations Between Indicators of Integration and Instability

	Elite Instability	Communal Instability
Communications Potential		
Static Measures:		
Mass communications index	.206	.574
Personal communications index	.272	.511
Commercial communications index	.226	.549
Per cent population in cities over 20,000 in 1967	.147	−.169
Urban primacy (ratio of largest to 2nd city), 1965	−.100*	−.253*
Improved roads per 1,000 square miles, 1966	.310	.042
Railroad track in miles per 100,000 square miles, c. 1965	.401	−.059
Dynamic Measures:		
Per cent change in radios in use, 1960–66	−.001	−.246
Per cent change in number of commercial vehicles, 1958–66	−.100	−.240
Per cent change in proportion population in cities over 20,000, 1955–65	−.382	−.351
Mass-Elite Gap		
Static Measures:		
Ethnic imbalance in elite representation, Independence	−.064	−.162
Ethnic imbalance in elite representation, 1967, or immediately preceeding coup	−.030	−.040
Enrollment in secondary school as per cent of age group, 1957	.235*	.029*
Enrollment in secondary school as per cent of age group, 1961	.259*	.011*
Enrollment in higher education as per cent of age group, 1961	.254*	.054*
Dynamic Measures:		
Average annual increase in secondary enrollment, 1957–61	−.171*	−.178*
Change in ethnic imbalance of elite representation, Independence to 1967	.103	.041
Pluralism		
Static Measures:		
Variance in ecological adaptation index	−.064	.434
Variance in stratification/authority index	.202	.227
Variance in social organization index	.091	.115
Per cent speakers of major vernacular language	−.170*	−.120*
Number of languages spoken	.332	.652
Linguistic homogeneity (ratio of number of speakers of dominant language to number of second)	−.230*	−.180*
Per cent population identifying with Islam, 1966	−.140*	.080*
Per cent population identifying with Christianity, 1966	.040	.070
Religious fractionalization index, 1966	−.284	.419
Religious homogeneity (ratio of number of identifiers with dominant universal religion to number identifying with other), 1966	−.271*	−.297*
Centralization		
Static Measures:		
Legislative fractionalization at Independence	.380*	.340*
Number of political parties between Independence to 1969	.460	.351
Wage labor force employed in the public sector, c. 1966	.381	.190
Central government revenue, 1963	.202	.537
Direct taxes as per cent of total government revenue, 1963	−.037	.275
Government revenue as per cent of GNP, 1963	−.035	−.287
Dynamic Measures:		
Rate of growth in government spending as per cent of GNP, 1963–65	.096	.250
Rate of growth in central government revenue, 1963–66	−.222	.139
Rate of growth in total government revenue as per cent GNP, 1963–65	−.109	.189
Coercive Potential		
Static Measures:		
Domestic coercive potential index	.254	.781
Military spending index	.305	.284
Foreign military aid index	.298	.573
Number of parties banned, 1957–69	.710	.542
Dynamic Measures:		
Per cent change in defense budget, 1963–67	.168	.167

* The signs on these correlations should be reversed to reflect the fact that the operationalization of the measure goes in the opposite direction to the category concept. i.e., *Increasing* level of the indicator is associated with *decreasing* level of the conceptual dimension being operationalized.

Table 3. Results of stepwise, multiple regression analysis of elite and communal instability using all indicators of integration, but permitting only six indicators to enter the regression equation

Independent Variable Entered	Regression Coefficients (b) Relating to Dependent Variables	
	Elite Instability	Communal Instability
Personal Communications (factor score)	−7.44**	n.e.
Improved Road Density, 1965	.07**	.03
Railroad Track Density, 1965	.01**	n.e.
Change in Urbanization, 1955–65	n.e.	−.01
Secondary Enrollment, 1957	n.e.	−.04**
Number of languages spoken	.09**	.11**
Government Revenue as % GNP, 1963	n.e.	−.19
Domestic Coercive Potential (factor score)	n.e.	16.07**
Military Spending (factor score)	7.62**	n.e.
Number of parties banned	.06	n.e.
Total Variance Explained (R^2)	61%	78%

n.e. = indicator not entered into the equation.

** Regression coefficient significant at the .01 level.

additional evidence discriminating between the processes of elite and communal instability. As the table shows, only two of the six variables in the equation for elite instability also enter the equation for communal instability. The precise nature of the differences in the processes of elite and communal instability, and the tests of our hypotheses, must, however, be based on a causal analysis in which the covariation of different patterns of integration is taken into account, and spuriously high correlations are minimized by partialing out, or controlling for, the unique variance in the dependent variables explained by different independent variables. The multiple regression equations in Table 3 do in fact partial out the variance explained by different indicators of integration, but the criteria for introducing variables into the equation have only to do with maximizing the explanation of variance, and they do not provide a basis for assessing whether or not different patterns of integration modify, or intervene in, the process of instability in the ways we have hypothesized.

Direct Causal Linkages. Before summarizing the evidence directly related to our hypotheses, three general statements about the results of the 486 path solutions for the elite and communal instability models can be made: (1) Compared to the explanations suggested by multiple regression equations, in which the explanation of variance in elite or communal instability is maximized by selecting from all 42 of our independent variables, the explanations provided by path analysis of our model are less impressive in terms of the statistical size of the variance explained. The constraints of causal analysis limit the explanation of political instability to a consideration of only one indicator of each of the five dimensions of integration. In the "best" explanation of elite instability in the causal tests of our model, 42 per cent of the variance in elite instability is accounted for by measures of integration. Correspondingly, the best fit model of communal instability explains 76 per cent of the variance in communal instability by measures of integration. This should be contrasted to the 61 per cent of elite instability and 78 per cent of communal instability accounted for in the results of the stepwise regression analysis shown in Table 3. (2) As in the results of the stepwise regression analysis, the causal analysis gives converging evidence for the differences in the structural processes leading to elite and communal instability. In the best-fit causal model of elite instability, the independent variables are those numbered 2, 13, 22, 30 and 38 in Table 1. In contrast, the independent variables in the best-fit model of communal instability are those numbered 10, 15, 19, 28, and 36. (3) Causal models based on static indicators of integration are more powerful statistically than are models based on dynamic measures. Nevertheless, as we shall indicate below, there are fundamental differences in the patterns of causal linkage between integration and instability when dynamic rather than static measures of integration are used. This confirms the suggestions made from a consideration of the zero-order correlations between indicators of integration and instability summarized in Table 2.

It will be useful, first, to summarize the evidence relating to the direct causal linkages in our model. Increasing communications potential, centralization, coercive potential, and decreasing mass-elite gaps and pluralism, are hypothesized to affect directly the incidence of political instability by decreasing the probability of elite and communal instability. (See hypotheses 1.1, 1.2, 2.1, 2.2, 3.1, and 3.2.) This is the most simple expression of the theory of integration and political instability, and the results necessary to the evaluation of these hypotheses are contained in Table 4.

The hypothesis stipulating negative direct effects for measures of communications potential on the incidence of political instability is supported by our results for dynamic measures of change in urbanization, but is directly contradicted by the effects of the static measure of the scale of mass communication on communal instability, and by the effects of the static measure of road density on both forms of instability. The direct effects of the measure of the scale of mass communications increase the probability of elite instability in 51 per cent of the tests, and increase the probability of communal instability in 88 per cent of the tests using this indicator. The direct effects of the measure of road density increase the likelihood of elite instability in 100 per cent of the tests, and increase the likelihood of communal instability 69 per cent of the time. The direct effects of the dynamic measure of increasing urbanization, however, decrease the likelihood of both elite and communal instability in *all* tests of these models, as hypothesized.

These anomalous, or contradictory, conclusions indicate the need for close attention to the effects of growth in the communications potential of political systems in future research. Our

Table 4. Direct Effects of Integration on Political Instability

		Elite Instability			Communal Instability		
Path	Independent Variable	% significant[a]	% positive[b]	Range[c]	% significant[a]	% positive[b]	Range[c]
P_{61}	A. Mass communications (factor score)	0	51	−.13 to .15	14	88	−.15 to .54
	B. Improved roads per 1,000 square miles	72	100	.17 to .53	0	69	−.19 to .48
	C. % Change in population in cities over 20,000	0	0	−.19 to −.03	88	0	−.64 to −.29
P_{62}	A. Enrollment in secondary school as % age group	5	100	.01 to .32	48	0	−.57 to −.01
	B. Change in ethnic imbalance of elite representation	5	100	.03 to .36	0	33	−.40 to .15
	C. Average annual increase in secondary enrollment	0	12	−.25 to .04	0	49	−.32 to .35
P_{63}	A. Variance in stratification/authority (factor score)	2	100	.05 to .28	5	67	−.08 to .35
	B. Number of languages spoken	47	100	.18 to .44	95	100	.27 to .77
	C. Religious fractionalization	0	100	.00 to .28	38	99	.07 to .56
P_{64}	A. Legislative fractionalization at independence	0	100	.10 to .25	26	0	−.53 to −.19
	B. Central government revenue	10	30	−.23 to .16	25	74	−.33 to .77
	C. Rate of growth in government spending as % GNP	0	41	−.13 to .11	4	85	−.11 to .46
P_{65}	A. Domestic coercive potential (factor score)	0	100	.03 to .25	100	100	.57 to .84
	B. Foreign military aid (factor score)	15	100	.06 to .33	72	100	.17 to .67
	C. % Change in defense budget	0	100	.05 to .16	0	86	−.07 to .12

[a] The percentage of all solutions of the path model using a particular indicator, in which the path from that indicator to the dependent variable was significant—i.e., in which the Z-score of the standardized partial regression coefficient was significant at the .05 level.

[b] The percentage of all solutions of the path model using a particular indicator, in which the standardized partial regression coefficient was positive.

[c] The range in the values of the standardized partial regression coefficient (β) for all solutions of the path model using a particular indicator.

results suggest the importance in African nations of the different effects of the level of communications potential on the one hand and the growth of communications potential on the other. They suggest also the importance of the effects of different kinds of communication potential on conflict and instability involving elite groups on the one hand and communal groups on the other. The indication that higher levels of communication potential increase the likelihood of both kinds of political instability, but that increasing growth in communications potential decreases the likelihood of both kinds of political instability, suggests that, unless there is a consistent increase in the communication between members of African political systems, higher levels of communication will increase rather than decrease the salience of conflicts among the parties in communication. Furthermore, all modes of communication do not affect all members of political systems equally. The level of mass communications, which allows for greater communication between members of communal groups and for greater awareness of their mutual differences, has a greater effect on the likelihood of communal instability. And the level of road density, which allows for greater communication between commercial and political elites who have the facilities for road travel, has a greater effect on the likelihood of elite instability than communal instability. Intelligent elaborations of these suggestions must await refinements in data and theory.

The hypothesis stipulating positive direct effects of measures of the size of the mass-elite gap on the incidence of political instability is supported by our results for the effects of dynamic measures on elite instability, and the effects of the static measure on communal instability. The hypothesis is, however, contradicted by the results for the effects of the static measure on elite instability, and there is no converging evidence for or against the hypothesized effects of the dynamic measures on communal instability. The direct effects of the static measure of secondary school enrollment increase the likelihood of elite instability in 100 per cent of the tests, but decrease the likelihood of communal instability in all tests using this indicator. The direct effects of the dynamic measure of increasing secondary school enrollment decrease the likelihood of elite instability in 88 per cent of the tests, but have no consistent tendency to decrease or increase the likelihood of communal instability. The direct effects of the dynamic measure of increasing ethnic imbalance of elite representation increase the likelihood of elite instability in 100 per cent of the tests, but decrease the likelihood of communal instability in 67 per cent of the tests using this indicator.

These conclusions are by no means clear-cut, but they do suggest that there are marked differences in the effects of the mass-elite gap on elite and communal instability. If we focus on the level of secondary school enrollment and change in the ethnic imbalance of elite representation, our results suggest a particular dilemma facing African political development. Those means of closing the mass-elite gap which succeed in reducing the incidence of elite instability increase the likelihood of communal instability, and those means which succeed in reducing the incidence of communal instability increase the likelihood of elite instability. Increasing the access to elite educational qualifications seems to inhibit the likelihood of communal instability, but to increase the likelihood of elite instability. The problem is one of satisfying the demands of ethnic groups for equal access to the rewards of modernization and elite status, while at the same time preventing an intensification of conflict over the scarce positions of power and wealth among members of an increasingly large educational elite in these nations. The dilemma is further underlined by the evidence that decreasing the ethnic imbalance in elite representation seems to inhibit the likelihood of elite instability, but to increase the likelihood of communal instability. The problem here is that orderly political development in the ethnically heterogeneous nations of Africa is probable only when a very delicate balance of ethnic representation in the political elite is maintained, such that there is sufficiently balanced representation to prevent alternative elites mobilizing the demands of underrepresented communal groups as justification for coups d'état, but not so exact a correspondence between ethnic proportions in the elite and total population that communal identities and demands are likely to override national identity and interests.

The hypothesis stipulating positive direct effects of pluralism on the incidence of political instability is confirmed. No matter how operationalized, the direct effects of pluralism on elite instability are positive in 100 per cent of our tests, and in practically all of the tests for communal instability. These results are perhaps no more than confirmation of common wisdom, but they do lend striking support to the theoretical importance of value congruence as a basis of political order, and they suggest the need for further research on dynamic processes of value change and assimilation, for which aggregate data are not presently available.

The hypothesis stipulating negative direct effects of centralization on the incidence of political instability is partially confirmed by the results for elite instability, but is directly contradicted by the results for communal instability. The direct effects of the measure of legislative fractionalization increase the likelihood of elite instability in 100 per cent of the tests, but decrease the likelihood of communal instability in all tests using this indicator. This reversal also holds for the measure of central government revenue, whose direct effects decrease the likelihood of elite instability in 70 per cent of the tests, but increase the likelihood of communal instability in 74 per cent of the tests using this indicator. The direct effects of the dynamic measure of growth in government spending are positive in 85 per cent of the tests for communal instability, but there is no convergent evidence that this measure increases or decreases the likelihood of elite instability.

Again, therefore, we are confronted with evidence for a problematic relationship between a dimension of national integration and elite and communal instability in African nations. Strong converging evidence suggests that centralization directly increases the likelihood of communal instability, but that the effects of at least two measures of centralization decrease the likelihood of elite instability. The problem is that centralization, on the one hand, increases the chances of conflict between central government decision makers and decision makers in the periphery of ethnically heterogeneous African political systems. Centralization threatens the authority of subnational political leadership, whose legitimacy in peripheral areas is likely to be greater than that of the distant leadership of the central government. On the other hand, however, centralization reinforces the power position of national elites and tends to decrease the likelihood of elite instability.

The hypothesis stipulating negative direct effects of coercive potential on the incidence of political instability is directly contradicted by our results, which show that all measures of coercive potential directly increase the likelihood of elite and communal instability. This finding is weakest when the dynamic measure of change in defense budget is used as an operationalization; in that case, the values of the path regression coefficients are low, but the direct effects on communal instability are positive in 86 per cent of the tests. In all other tests for both elite and communal instability, the direct effects of coercive potential are strongly positive.

Further evidence discriminating between the processes of elite and communal instability is provided by these results, which show that the direct effects of coercive potential are much more likely to be statistically significant for communal instability than for elite instability. In addition, it is clear that the measure of domestic coercive potential has the most powerful direct effects on communal instability (the path regression coefficients range from .57 to .84), whereas the measure of foreign military aid has the strongest direct effects on elite instability. This finding can again be explained in terms of the distinctions between elite and communal groups as actors in the different processes of instability. Domestic coercive potential is visible to mass publics, and used against them. Foreign military aid is visible to elites, and used by them. That coercive potential increases rather than decreases political instability can be explained in several ways: (1) a long-term resistance to coercion characterizes most human situations; (2) the range of the application of coercion is limited in African nations by the small size of most security forces; (3) there is a lack of institutionalization in police and armed forces, which have inherited the distasteful record of colonial repression; and (4) the same lack of institutionalization characterizes national political authority, making it more likely that in situations of increased coercive potential, men, especially military men, will take power rather than take orders.

The hypotheses (3.1 and 3.2) relating to the differential importance of the direct effects of the mass-elite gap and pluralism on the incidence of elite and communal instability tend to be confirmed. There is a good deal of variation in the directionality of the direct effects of the mass-elite gap on the incidence of communal instability, except when the static measure is used, and the values of the regression coefficients for P_{62} in the communal instability model range widely, but tend to be close to zero. In the elite instability model, the direct effects of the mass-elite gap, no matter how operationalized have clear-cut directionality, and the values of the coefficients for P_{62}, although not strikingly large, range less widely and are less often close to zero than in the communal instability model. Tentatively, therefore, we confirm our expectation that the size of the mass-elite gap has more to do with the incidence of elite instability than with the incidence of communal instability. In addition, pluralism is directly and positively associated with both forms of instability, but the values of the regression coefficient for P_{63} are consistently greater in the tests of the communal instability model than in the corresponding tests for elite instability, and the scores of the coefficients are more often significant in the analysis of communal instability than in the analysis of elite instability. We conclude, therefore, that pluralism affects communal instability more directly than it affects elite instability.

Interactive Causal Linkages. It was hypothesized that increases in centralization and coercive potential critically intervene in the relationships between communications potential, the mass-elite gap, pluralism, and political instability of both kinds. These indirect effects of centralization and coercive potential were further hypothesized to reduce the liklihood that elite or communal instability would result from processes of horizontal, vertical and value integration. (See hypotheses 1.3, 1.4, 3.3, 3.4, and 3.5.) The results necessary for the evaluation of these hypotheses are summarized in Table 5.

The hypothesis that the direct effects of communications potential are critically affected by, or negligible as a result of, the indirect effects of centralization and coercive potential is generally confirmed. When communications potential is operationalized as the scale of mass communications, there are, as hypothesized, well-identified paths between this measure and coercive potential (P_{51}) in all tests of the elite and communal instability models. When communications potential is operationalized as road density, there are relatively pronounced paths between this measure and measures of centralization (P_{41}) in most of the tests of the elite and communal instability models. And when communications potential is operationalized as increasing urbanization, there are also relatively pronounced paths between this measure and measures of either coercive potential or centralization. Since the paths between coercive potential and both kinds of political instability (P_{65}) have already been consistently identified in Table 4, and since the paths between coercive potential and centralization (P_{54}) are relatively pronounced in the results in Table 5, there is evidence that the effects of communications potential on the likelihood of political instability are critically affected by either the intervening effects of coercive potential alone, or by the intervening effects of coercive potential and centralization in combination.

However, the hypothesis that these indirect effects of centralization and coercive potential decrease the probability that political instability will result from communications potential is directly contradicted by most of our results. When the index of the scale of mass communications is used to operationalize communications potential, the paths linking this measure with coercive potential and political instability (P_{65} and P_{51}) are consistently positive in both the elite and communal instability models. In this case, therefore, the intervening effects of coercive potential intensify the already identified tendency for mass communications to increase the likelihood of political instability. When communications potential is operationalized as road density, the paths linking this measure with measures of centralization, coercive potential and political instability (P_{65}, P_{54} and P_{51}) are all positive in more than 70 per cent of the tests, again contradicting the hypothesis. Only when we operationalize communications potential in terms of increasing urbanization are the products of the intervening paths ($P_{65} \cdot P_{51}$ or $P_{65} \cdot P_{54} \cdot P_{51}$) more often negative than positive in the tests for elite instability, but there is no converging evidence for the directionality of these paths in the tests of the communal instability model.

The hypothesis that the direct effects of the mass-elite gap are critically influenced by the indirect effects of centralization and coercive potential is generally confirmed. The values of the path regression coefficients for the paths linking the mass-elite gap to centralization (P_{42}) and coercive potential (P_{52}) tend to be high in the tests for both elite and communal instability. There is, however, no converging evidence that the indirect effects of coercive potential and centralization increase or decrease the likelihood that political instability will result from the mass-elite gap, and we cannot, therefore, confirm the hypothesis to that effect. The results for P_{52} indicate that the indirect effects of coercive potential increase the likelihood that both forms of political instability will result from the mass-elite gap when dynamic measures of secondary school enrollment are used as an operationalization; but they take the hypothesized direction when dynamic measures of change in the ethnic imbalance of the elite are used. The results for P_{42} show that the indirect effects of centralization tend to decrease the likelihood that both forms of instability will result from changes in the ethnic imbalance of the elite, but the directionality of P_{42} is more or less random for other operationalizations of the mass-elite gap. The most we can say, therefore, is that there is very weak support for the hypothesis that the indirect effects of centralization and coercive potential de-

Table 5. Indirect Effects of Centralization and Coercive Potential

Path	Independent Variable	Elite Instability			Communal Instability		
		% significant[a]	% positive[b]	Range[c]	% significant[a]	% positive[b]	Range[c]
P_{51}	A. Mass communication index	44	100	.07 to .79	32	99	.02 to .70
	B. Improved roads per 1,000 sq. miles, c. 1965	5	33	−.40 to .16	0	36	−.39 to .20
	C. % Change in population in cities over 20,000, 1955–65	2	19	−.33 to .26	0	69	−.10 to .45
P_{52}	A. Enrollment in secondary school as % age group, 1957	14	44	−.29 to .47	25	88	−.05 to .59
	B. Change in ethnic imbalance of elite representation	5	27	−.46 to .21	0	33	−.45 to .36
	C. Average annual increase in secondary enrollment, 1957–61	1	9	−.33 to .22	9	7	−.48 to .17
P_{53}	A. Variance in stratification/authority index	15	98	−.02 to .44	10	98	−.05 to .43
	B. Number languages spoken, c. 1966	30	83	−.31 to .49	0	49	−.37 to .19
	C. Religious fractionalization index, c. 1966	36	100	.09 to .49	0	95	−.06 to .38
P_{54}	A. Legislative fractionalization at independence	2	16	−.34 to .19	0	52	−.13 to .35
	B. Central government revenue, 1963	44	83	−.08 to .73	33	73	−.08 to .65
	C. Rate of growth in govt. spending as % GNP, 1963–66	20	100	.01 to .44	0	99	.01 to .44
P_{41}	A. Mass communications index	33	46	−.19 to 1.17	33	41	−.15 to 1.08
	B. Improved roads per 1,000 square miles, c. 1965	6	70	−.35 to .58	4	77	−.38 to .43
	C. % Change in population in cities over 20,000, 1955–65	7	38	−.52 to .11	4	33	−.51 to .15
P_{42}	A. Enrollment in secondary school as % age group, 1957	26	46	−.21 to 1.47	26	44	−.19 to 1.19
	B. Change in ethnic imbalance of elite representation	15	14	−.55 to .47	16	14	−.71 to .31
	C. Average annual increase in secondary enrollment, 1957–61	59	44	−.39 to .55	42	46	−.40 to .55
P_{43}	A. Variance in stratification/authority index	19	83	−.10 to .78	17	90	−.05 to .79
	B. Number languages spoken, c. 1966	33	40	−.16 to 1.49	33	63	−.19 to 1.36
	C. Religious fractionalization index, c. 1966	19	100	.02 to .49	6	95	−.05 to .46

[a] Z-score significant at the .05 level.

[b] The percentage of all solutions of the path model using a particular indicator, in which the standardized partial regression coefficient was positive.

[c] The range in the values of the standardized partial regression coefficient (β) for all solutions of the path model using a particular indicator.

crease the probability that political instability will result from increasing ethnic imbalance in the elite.

The hypothesis that the direct effects of pluralism are critically affected by the indirect effects of centralization and coercive potential is confirmed, but the hypothesis that these indirect effects decrease the probability that political instability will result from pluralism is clearly contradicted by our results. Except for the slightly ambiguous results when pluralism is operationalized as the number of languages spoken in a given nation, the paths between pluralism and coercive potential (P_{53}) and between pluralism and centralization (P_{43}) are consistently positive, and the values of the path regression coefficients tend to be high. The joint effects, therefore, of greater pluralism, centralization and coercive potential exacerbate, rather than inhibit, the incidence of both communal and elite instability.

The results in Table 5 require a rejection of the hypothesis that political centralization and coercion are likely to redress the strain or conflict resulting from the mass-elite gap or cultural pluralism in African nations, or that these procedures are likely to regulate conflicts which may be intensified by increasing communications between previously segregated actors in these nations. Much more detailed research is needed in this area, given the incompleteness of our operationalizations of centralization and coercion, and the rather less consistent results for the indirect effects as opposed to the direct effects in our models of elite and communal instability. Nevertheless, our data suggest that political conflict in contemporary African nations will be intensified by political centralization so long as the financial demands of national authorities exceed their responsiveness to local constituencies, and so long as coercion is emphasized by governments in their response to political demands. With regard to coercion, Gurr has written:

> The most fundamental human response to the use of force is counterforce. Force threatens and angers men, especially if they believe it to be illicit or unjust. Threatened, they try to defend themselves; angered, they want to retaliate. . . . There are only two inherent limitations on the escalating spiral of force and counterforce; the depletion of either group's resources for coercion, or the attainment by one of the capacity for genocidal victory over its opponents.[57]

These limitations do not exist in African nations, except perhaps in a case like Nigeria, where they were attained only at the cost of exceptionally bitter civil war. In general, however, the coercive capability required to repress political violence successfully is not available to African states. And short of that capability, greater coercive potential means a greater potentiality that those who have guns will use them, and that those who have guns used against them will retaliate.

Conclusions

In conclusion we will summarize the following aspects of the preceding sections: (1) problems associated with the empirical validation of the theory of national integration as we have outlined it; (2) the empirical verification of the difference we hypothesized between models of elite and communal instability; (3) the implications of our results for an assessment of the basic hypothesis in integration theory that there is a uniform, positive relationship between integration and stability.

(1) We stressed in the section on the theory of integration and instability that, unless the major concepts defining integration were empirically unidimensional, the hypotheses relating integration to instability were likely to hold up only on some operationalizations and not on others. Our operationalizations of national integration proved to be multidimensional, and to have complex relationships with measures of political instability. This conclusion suggests four general problems associated with the empirical validation of the theory of national integration. The first two problems are primarily theoretical; the last two primarily methodological.

In the first place, theoretical work on national integration will have to take account of the empirical multidimensionality of the major concepts and to make explicit the hypothetical relationships and significance of different kinds of horizontal, vertical and value integration, political centralization and coercion. Second, our results suggest that some of the disputes in the literature on integration—for example, the question of whether increasing communication leads to stability or instability—result in part from a failure to identify the significance of different kinds of interaction for different political actors like elites and communal groups. Third, the acknowledgement of the multidimensionality of most concepts in the theory of national integration makes the evaluation of causal models derived from this theory extremely difficult. In this paper, we have dealt with only a fraction of all possible operationalizations of our models of integration and political instability, but even so, the richness of the data is counterbalanced by the complexity of summary explanation. Finally, the tests of our hypotheses sharply diverge depending on whether static or dynamic measures of integration are used, and it is important that future research be directed toward explicating the dynamics and nonlinearities in models relating integration to instability.

(2) Our theory was built on a very explicit hypothesis that integration is related to elite

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[57] Gurr, *Why Men Rebel,* p. 24.

and communal instability in different ways. The results of both the correlational and causal analysis have demonstrated the clear dichotomy that was hypothesized. The analysis of bivariate correlations and of multiple regression equations indicated that very few measures of integration predicted equally well to our measures of elite and communal instability. Similarly, the path analysis indicated substantial differences in the major causal linkages between dimensions of integration and elite instability on the one hand, and communal instability on the other. These results again emphasize the importance of considering national integration in the context of conflicts between different political actors.

(3) The basic assumption of integration theory that there is a strong, negative relationship between national integration and political instability needs considerable qualification in the light of our results for independent black African nations. We do find substantial support for the hypothesis that value congruence is a fundamental condition of political order in developing African nations, and that higher levels of ethnic pluralism, religious fractionalization, and linguistic heterogeneity are related to higher probabilities of both elite and communal instability.

These results should not, however, be taken to imply the deterministic inevitability of political instability in pluralistic African nations.[58] Our data on pluralism are cross-sectional, and the critical question, still to be answered for Africa, is to what extent dynamic processes of value integration, or assimilation, inhibit the incidence of political instability. We have not explored the effects of different styles of political organization and ideological mobilization, or rates of change in education and religious conversion, on the relationship between pluralism and instability.[59] Our results, nevertheless, indicate the importance of looking at value integration as a variable attribute of nations which has an important effect on their political instability.

We find, further, partial support for the theoretical expectation that increasing horizontal and vertical integration decrease the likelihood of political instability. Increased urbanization decreases the likelihood of both elite and communal instability in black African nations. We must be cautious in generalizing this result to all forms of increasing horizontal integration, but the bivariate correlations indicate converging evidence for similar relationships among at least three different measures of communications potential growth and political instability. Our data also indicate that increasing the access to secondary education and decreasing the ethnic imbalance of the ruling elite inhibit the occurrence of elite instability in these nations.

This partial confirmation of the theory must, however, be qualified by the contradictory findings that higher levels of mass communication increase the probability of communal instability; higher levels of road density increase the probability of both kinds of political instability; decreasing the ethnic imbalance in the ruling elite increases the likelihood of communal instability, and higher levels of secondary school enrollment increase the likelihood of elite instability. Insofar as these contradictory results are related to the use of static as opposed to dynamic measures of integration, they suggest the following explanation: Unless the process of integration is dynamically persistent, higher levels of integration provide for greater communication between disparate social groups, and increase the likelihood that their mutual conflicts, which have been inhibited by a past history of increasing contact and cooperation, will increase in intensity. Increasing integration, therefore, may tend to promote political stability, but it also brings closer to the political arena those conflicts that have not been sufficiently well resolved in the past. As van den Berghe puts it: ". . . while societies do indeed show a tendency towards stability, equilibrium, and consensus, they simultaneously generate within themselves the opposite of these."[60]

Finally, there is no support in our analysis for the theoretical argument that political centralization and coercion decrease the likelihood of political instability in black African nations. Political centralization and coercion do critically affect the relationships of horizontal, vertical and value integration with political instability, but the direction of these effects is consistently in the opposite direction to that hypothesized. The dispute in the literature over the significance of values as opposed to coercion as the basis of political order is not, of course, definitively resolved by our results. But our evidence for this sample of nations favors the former rather than the latter view.

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[58] For more pessimistic conclusions, see Edward Feit, "Military Coups and Political Development: Some Lessons from Ghana and Nigeria," *World Politics*, 20 (January, 1968), 179–193, and J. O'Connell, "The Inevitability of Instability," *Journal of Modern African Studies*, 5 (1967), 181–191.

[59] Some of the important considerations left out of this analysis are the effects of modernization on the relationship between cultural pluralism and political instability. We have investigated that problem in our "Cultural Pluralism, Modernization and Conflict: Sources of Political Instability in Black Africa," *Canadian Journal of Political Science* (March, 1972).

[60] Pierre van den Berghe, "Dialectic and Functionalism: Toward a Theoretical Synthesis," *American Sociological Review*, 28 (October, 1963), 696–697.

This analysis of integration in African nations may be discouraging. Discouraging to those who greatly desire national unity and political order in Africa, and discouraging to those who enjoy parismonious theoretical explanations of political processes. But the dream of unity is unlikely to be realized in the decade after independence, and the development of political order in Africa cannot be explained satisfactorily by models and theories developed from the contemporary description of long-established and economically developed nations. Integration must be placed in the context of a lengthy process of political development, in which national political systems have to face problems of the challenge of modernity, consolidation of leadership, and economic and social transformation, before they experience the stabilizing effects of integration.[61] Karl Deutsch argues:

> As a political process, integration has a *take-off point* in time, when it is no longer a matter of a few prophets or scattered and powerless supporters, but turns into a larger and more coordinated movement with some significant power behind it. Before take-off a proposal for integration is a theory; after take-off, it is a force.[62]

We may say that before take-off, a theory of integration is a proposal; after take-off, it has explanatory force. It is, nevertheless, clear from this analysis that different processes of integration have important effects on the instability of developing African political systems. Even when the coefficients are small, these relationships should not be interpreted as unimportant, for *pace* William James, a difference that makes a difference *is* a difference.

Appendix

As indicated in the section on methodology in this paper, we have used factor analysis as a technique for combining in a single index the scores of a number of different variables that are theoretically and empirically associated with the same underlying conceptual phenomenon. We have used this technique to develop measures of communications potential, cultural pluralism and coercive potential. A large number of variables that were presumed *a priori* to indicate different aspects of these conceptual categories were subjected to an initial factor analysis, using an orthogonal solution, and a varimax rotation of the factors so extracted. We obtained in this way evidence for three independent dimensions of each of the original conceptual categories, which we labeled mass communications, personal communications and commercial communications; cultural pluralism in the patterns of ecological adaptation, authority and stratification, and social organization of ethnic units within nations; domestic coercive potential, foreign military aid and military spending. By inspection of the factor loadings in the rotated factor solutions, we then selected variables with a distinct contribution to these dimensions, and separately factor analyzed each of these sets of variables. If, as proved to be the case, the unrotated solution for each set of variables produced a single, unidimensional factor, explaining more than 80 per cent of the variance in the correlation matrix for that set of variables, we used the factor scores from that factor to measure the dimension already labeled.

The factors indicating dimensions of communications potential are:

A. *Mass Communications*

Variable	*Factor Loading*
Number of telephones in use in 1954	.802
Number of telephones in use in 1962*	.923
Number of telephones in use in 1964	.930
Number of telephones in use in 1966*	.931
Number of radio sets in use in 1964	.924
Number of radio sets in use in 1966	.825
Circulation of daily newspapers 1955	.923
Circulation of daily newspapers 1964	.935
Number of daily newspapers in 1964	.838
Number of cinemas in 1965	.825
Seating capacity of cinemas in 1965	.838
Number of book publishing companies, 1968	.887

B. *Personal Communications*

Variable	*Factor Loading*
Number of telephones in use in 1962*	.964
Number of telephones in use in 1966*	.941
Number of automobiles in 1958	.943
Number of automobiles in 1962	.978
Number of automobiles per capita in 1964	.966
Number of automobiles per capita in 1966	.984

* These measures are included in both of the above factors because they loaded above .50 on two factors in the original factor analysis of measures of communications potential.

C. *Commercial Communications*

Variable	*Factor Loading*
Number of commercial vehicles in 1958	.978
Number of commercial vehicles in 1960	.977

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[61] See C. E. Black, *The Dynamics of Modernization: A Study in Comparative History* (New York: Harper and Row, 1966).

[62] Deutsch, *The Analysis of International Relations*, p. 198.

Number of commercial vehicles in 1962	.988
Number of commercial vehicles in 1964	.981
Number of commercial vehicles in 1966	.925
Number of miles of railroad track in 1965	.825

The factors indicating dimensions of cultural pluralism are:

A. *Variance in the Ecological Adaptation of Ethnic Units*

Variable	*Factor Loading*
Variance in the extent of dependence on animal husbandry	.908
Variance in the extent of dependence on agriculture	.787
Variance in the intensity of agricultural exploitation	.864
Variance in the complexity and stability of settlement patterns	.885

B. *Variance in the Authority and Stratification Patterns of Ethnic Units*

Variable	*Factor Loading*
Variance in the number of levels of hierarchy above the family	.808
Variance in the number of levels of hierarchy in local community organization	.893
Variance in the degree of class stratification	.825
Variance in patterns of succession to local office	.836
Variance in patterns of distribution of inheritance	.689
Variance in rules for inheritance of movable property	.756

C. *Variance in Social Organization of Ethnic Units*

Variable	*Factor Loading*
Variance in location of marital residence	.826
Variance in structure of community organization	.760
Variance in extent of patrilineality	.850
Variance in extent of matrilineality	.826
Variance in principles of reckoning descent	.966

The factors indicating dimensions of coercive potential are:

A. *Domestic Coercive Potential*

Variable	*Factor Loading*
The size of total armed forces in 1964	.909
The size of total armed forces in 1966	.950
The size of total armed forces in 1967	.924
The size of the army in 1966	.939
The size of the air force in 1966	.929
The size of police and gendarmerie in 1964	.958
The size of police and gendarmerie in 1966	.976
The size of police and gendarmerie in 1967	.955

B. *Foreign Military Aid*

Variable	*Factor Loading*
The number of countries giving military aid, c. 1965–66	.955
The amount of total military aid, 1965–66	.981
The amount of total military aid from Western nations, 1965–66	.923

C. *Military Spending*

Variable	*Factor Loading*
Defence Spending as percentage of GNP, 1963	.904
Defence Spending as percentage of GNP, 1965*	.864
Defence Spending as percentage of GNP, 1965*	.612
Defence Spending as percentage of GNP, 1966	.508
Defence Spending as percentage of GNP, 1967*	.900
Defence Spending as percentage of GNP, 1967*	.902
Defence Expenditure as percentage of total government spending, 1966	.860

* These variables were coded from two different sources.

The factor analysis results reported in this appendix were obtained using the computer program MESA1. This program, originally written at the University of Chicago and now in the program library of the Institute for Behavioural Research at York University, employs a principal-components factor analysis routine. We used 1.00 as the communality estimates in the diagonal of the correlation matrix, and the criterion for the number of factors rotated was based on an examination of the discontinuities in the eigenvalues, following Robert L. Linn, "A Monte Carlo Approach to the Number of Factors Problem," *Psychometrika*, 33 (March, 1968), 37–41.

A Test of a Partial Theory of Potential for Political Violence*

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Collective violence has been endemic in American history. But specifically *political* violence, that is, violence directed against the regime (the structure of political authority) and/or against particular authorities occupying positions in the regime,[1] apparently has been most acute during the recent decade.[2] The prevailing explanation[3] in the social science literature accounts for political violence as a function of discrepancy between what men desire to achieve and what they perceive they actually have achieved or will be able to achieve.[4]

The most comprehensive and systematic elaboration of this explanation to date appears in the work of Ted Gurr.[5] His concept of discrepancy between achievement optimum and achievement is labeled "relative deprivation." In sociological research the term "relative deprivation" commonly denotes discrepancy between some status or values that an individual thinks he *should* have *in relation to the status or values of a reference group,* and the status or values which he perceives he actually does have.[6] However, since value standards may have sources other than reference groups, Gurr proposes, following David F. Aberle, a more general definition of relative deprivation, as actors' perceptions of discrepancy between their value expectations (the goods and conditions of life to which they believe they are justifiably entitled) and their value capabilities (the amounts of those goods and conditions that they think they are able to get and keep).[7] Val-

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[1] This definition follows that given in Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970), pp. 3–4.

[2] See Ivo K. Feierabend, Rosalind L. Feierabend, and Betty A. Nesvold, "Social Change and Political Violence: Cross-National Patterns"; Sheldon G. Levy, "A 150-Year Study of Political Violence in the United States"; and Philip Taft and Philip Ross, "American Labor Violence: Its Causes, Character, and Outcome"; in *Violence in America: Historical and Comparative Perspectives,* ed. Hugh Davis Graham and Ted R. Gurr (New York: Signet Books, 1969).

[3] Cf. Don R. Bowen, Elinor Bowen, Sheldon Gawiser, and Louis H. Masotti, "Deprivation, Mobility, and Orientation Toward Protest of the Urban Poor," in *Riots and Rebellion: Civil Violence in the Urban Community,* ed. Louis H. Masotti and Don R. Bowen (Beverly Hills: Sage, 1968); Thomas J. Crawford and Murray Naditch, "Relative Deprivation, Powerlessness, and Militancy: The Psychology of Social Protest," *Psychiatry,* 33 (May, 1970), 208–223; James C. Davies, "Toward a Theory of Revolution," *American Sociological Review,* 27 (February, 1962), 5–19; Ivo K. Feierabend and Rosalind L. Feierabend, "Aggressive Behaviors Within Politics, 1948–1962: A Cross-National Study," *Journal of Conflict Resolution,* 10 (September, 1966), 249–271; Ivo K. Feierabend et al., "Social Change and Political Violence: Cross-National Patterns," and James C. Davies, "The J-Curve of Rising and Declining Satisfaction as a Cause of Some Great Revolutions and a Contained Rebellion," in *Violence in America,* ed. Graham and Gurr; the work of Ted Gurr cited in footnote 5 below; Denton E. Morrison, "Some Notes Toward Theory on Relative Deprivation, Social Movements, and Social Change," *American Behavioral Scientist,* 14 (May, June, 1971), 675–690; and a study which does not bear on political violence directly, but rather, on support for political institutions, Samuel C. Patterson, G. R. Boynton, and Ronald D. Hedlund, "Perceptions and Expectations of the Legislature and Support for it," *American Journal of Sociology,* 75 (July, 1969), 62–76.

[4] As will be discussed at length in a subsequent report, this explanation actually comprises a number of separate theories, distinguished by different definitions of an individual's achievement optimum, i.e., the standard which defines his desirable achievement level. See Bernard N. Grofman and Edward N. Muller, "The Strange Case of Relative Gratification and Potential for Political Violence: The V-Curve Hypothesis," Department of Political Science, State University of New York at Stony Brook, ditto, 1972.

[5] The works central to the development and testing of his theory are: "Psychological Factors in Civil Violence," *World Politics,* 20 (January, 1968), 245–278, "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review,* 62 (December, 1968), 1104–1124, "A Comparative Study of Civil Strife," and *Why Men Rebel.*

[6] See the discussion in Gurr, *Why Men Rebel,* pp. 24–30.

[7] Gurr, p. 24; also see David F. Aberle, "A Note on Relative Deprivation Theory as Applied to Millenarium and Other Cult Movements," in *Millennial Dreams in Action: Essays in Comparative Study,* ed. Sylvia L. Thrupp (The Hague: Mouton, 1962).

ues, in turn, can be categorized into at least two major types of classes: (1) Welfare values—"those that contribute directly to physical well-being and self-realization"; (2) Power values—"those that determine the extent to which men can influence the actions of others and avoid unwanted interference by others in their own actions."[8] Collective violence or civil strife (of which political violence is a subset), is defined by three principal forms: (1) a Turmoil dimension—"characterized by largely spontaneous strife such as riots and demonstrations"; (2) a Conspiracy dimension—"typically including plots, mutinies, and most coups"; and (3) an Internal War dimension—"typically including civil war, guerrilla war, and some coups."[9]

The master assumption of the relative deprivation explanation is that "a psychological variable, relative deprivation, is the basic precondition for civil strife of any kind."[10] The causal mechanism posited as underlying the deprivation-violence relationship is derived from frustration-aggression theory.[11] According to Berkowitz's formulation, perception of frustration (interference with goal-directed activity) arouses an emotional state or drive labeled "anger." A readiness for aggressive behavior is an innate response to anger; overt aggressive behavior will occur in the presence of stimuli associated with the source of frustration.[12] In the relative deprivation explanation, frustration produced by perception of discrepancy between value expectations and value capabilities is the source of potential for political violence which, in the presence of suitable cues, will be expressed in overt behavior. The central proposition is that magnitude of civil strife (including specifically political violence) will vary strongly and directly with the degree, or intensity, of relative deprivation.

Gurr has hypothesized further that several variables may intervene in the deprivation-violence relationship. These are Coercive Potential, Institutionalization, Facilitation, and Legitimacy.[13] On the basis of a test of his theory with cross-national aggregate data, Gurr proposes that Facilitation is the only variable which substantially intervenes between deprivation and political violence. Two relative deprivation measures, Persisting Deprivation and Short-term Deprivation, are proposed to affect political violence directly, regardless of the above intervening variables.[14]

But developing an operational system based on macro indicators is not the most felicitous way to begin testing a theory conceptualized largely in terms of psychological characteristics of individuals.[15] Even if there were macro indicators that would permit reasonably valid inferences about the psychological variables presumed to affect political violence, tests based on such macro indicators would be most useful in the theory confirmation process after the theoretical propositions had been subjected to intensive micro analysis, so that the dynamics of the behavior of individuals were thoroughly understood.

The major proposition of the deprivation explanation has not found strong support in the few tests at the micro-level that have begun to appear, using a deprivation measure recommended by Gurr (the Cantril Self-Anchoring Striving Scale).[16] This may be because the

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[8] The quotations are from Gurr, *Why Men Rebel,* p. 25. Gurr also identifies an additional category, "interpersonal values," but some of these values overlap with the self-realization component of welfare values.

[9] The quotations are from Gurr, *Why Men Rebel,* p. 10. Political violence comprises most of the events which make up civil strife and the variables used to explain civil strife appear in one form or another as antecedents of political violence in *Why Men Rebel.*

[10] Gurr, "A Causal Model of Civil Strife . . . ," p. 1104.

[11] A detailed discussion is presented in Gurr, "Psychological Factors in Civil Violence," pp. 247–257; also see *Why Men Rebel,* pp. 30–37.

[12] See Leonard Berkowitz, "The Concept of Aggressive Drive: Some Additional Considerations," in *Advances in Experimental Social Psychology,* Vol. II, ed. Leonard Berkowitz (New York: Academic Press, 1965). However, Berkowitz is careful to point out, at page 308, that frustration "creates only a *readiness* for aggressive acts. Previously acquired aggressiveness habits can also establish this readiness."

[13] See Gurr, "A Causal Model of Civil Strife . . . ," pp. 1104–1106.

[14] See Figure 5 in Gurr, "A Causal Model of Civil Strife . . . ," p. 1121.

[15] These are Persisting Deprivation, Short-Term Deprivation, Legitimacy, the Communist Party Status component of Facilitation (a scale "based on the premise that illegal parties are more facilitative of strife because their membership is likely, because of the exigencies of repression, to be more dedicated, better organized, and committed to the more violent forms of conflict"), and the Past Strife Levels aspect of Facilitation (a scale predicated on the assumption that "populations in which strife is chronic tend to develop, by an interaction process, a set of beliefs justifying violent responses to deprivation"); the quotations are from "A Causal Model of Civil Strife . . . ," pp. 1114 and 1106, respectively.

[16] Micro-level tests are reported in Crawford and Naditch, and Bowen et al. In the Crawford and Naditch study, relative deprivation with respect to the present appears to be associated to a moderate degree with a measure of belief in the efficacy of past violence (Do you think that riots help or hurt the Negro cause?). The relative deprivation measure, however, shows what appears to be a weak association with a measure that one might assume would be related to potential for political violence (Will force or persua-

Cantril scale measures discrepancy between an individual's perception of his *best possible* achievement level and his actual achievement, whereas Gurr's formulation of the relative deprivation concept involves an individual's perception of discrepancy between that achievement level to which he believes he is *justifiably entitled* (value expectations) and his actual achievement (value capability). Thus, if individuals' perceptions of their best possible achievement level were poorly correlated with their perceptions of the level to which they are justifiably entitled, then findings based on the Cantril scale would be irrelevant to the relative deprivation explanation. On the other hand, if it is assumed that best possible achievement level will probably correlate (though by no means perfectly) with justifiable achievement level, then it would be reasonable to generalize from Cantril scale findings to relative deprivation propositions.

An alternate approach accounts for potential for political violence as a function of deep-rooted, generalized affect toward the political authority structure. Such affect has been labeled "diffuse support" by David Easton and "political trust" by William Gamson.[17] Following Easton, we may posit three dimensions of diffuse support for the political authority structure: (1) endorsement of the legitimating ideology of the regime, or the values and norms which constitute its rationale; (2) convictions that regime structures are worthy institutions—are valuable because of ethical reasons, because they have performed well over a long period of time, or just because they are there; (3) trust and confidence in the public officials who wield the power of the regime.[18] These dimensions of diffuse support refer to legitimacy sentiment which derives from "underlying ideological principles, from attachment to the structure and norms of the regime as such, or from devotion to the actual authorities themselves because of their personal qualities."[19] Easton calls such legitimacy sentiment "ideological legitimacy," "structural legitimacy," and "personal legitimacy." Gamson's elaboration of the political trust concept closely parallels Easton's diffuse support concept.[20]

The master assumption of the diffuse support explanation is that discontent stemming from generalized negative affect toward the political authority structure produces a readiness to engage in political violence. The rationale for this assumption may be sketched as follows. Children acquire legitimacy sentiment at an early age. If we assume that learning proceeds according to the "terracing" theory (later learning overlays rather than replaces early learning, and the earlier the learning, the more lasting the effect on later behavior), then legitimacy-illegitimacy sentiment will have a strong effect on adult behavioral support for the regime, a major component of which is readiness to avoid or to engage in political violence.[21] Furthermore, it might be predicted that, among citizens in general, the dimension of legitimacy sentiment which would have the greatest effect on potential for political violence would be that which would require the least cognitive ability and therefore would have been learned at the earliest age. This is the personal dimension of legitimacy: generalized trust and confidence in the political authorities.[22]

A pilot study, conducted as a preliminary to this project, provided empirical support for the proposition that potential for political violence is strongly correlated with legitimacy sentiment. Among a sample of college students, the multiple correlation between the dimensions of legitimacy sentiment and Avoidance of Protest Action was .474; and personal legitimacy—trust

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sion be necessary to change white attitudes?). In the Bowen et al. study, relative deprivation with respect to the present shows weak association with Protest Orientation. This team also finds that estimates of future deprivation, present-future upward mobility (decreasing discrepancy), and present-future downward mobility (increasing discrepancy) are all unrelated to Protest Orientation. Finally, upward and downward mobility types are lumped together as one category and individuals reporting no mobility are lumped together as another; this mobility dichotomy does show what appears to be a weak-to-moderate association with Protest Orientation, but the correspondence between the mobility variable and the context of meaning of the deprivation concept is not spelled out clearly.

[17] See David Easton, *A Systems Analysis of Political Life* (New York: Wiley, 1965), pp. 267–340; for empirical findings pertaining to the development of diffuse support see David Easton and Jack Dennis, *Children in the Political System: Origins of Political Legitimacy* (New York: McGraw-Hill, 1969). Also see William A. Gamson, *Power and Discontent* (Homewood, Illinois: Dorsey Press, 1968), esp. pp. 39–52.

[18] See Easton, pp. 278–340; also see the discussion in Edward N. Muller, "Correlates and Consequences of Beliefs in the Legitimacy of Regime Structures," *Midwest Journal of Political Science,* 14 (August, 1970), 392–412.

[19] Easton, "A Systems Analysis . . . ," p. 287.

[20] At page 45 of *Power and Discontent,* Gamson equates "political trust" with Easton's definition of "diffuse support."

[21] See the discussion of learning theory in Richard M. Merelman, "The Development of Political Ideology: A Framework for the Analysis of Political Socialization," *American Political Science Review,* 63 (September, 1969), 750–767.

[22] See Easton and Dennis, pp. 73–313.

in the political authorities—was the most potent single predictor.[23] Additionally, for a sample of black residents of Detroit, Michigan, Joel Aberbach and Jack Walker found that 54 per cent of those at the lowest ordinal position on a measure of trust in the political authorities reported that they could imagine a situation in which they would riot, whereas only 17 per cent of those at the highest ordinal position said they could imagine a situation in which they would riot.[24]

Another explanation of potential for political violence may be derived from the social learning theory elaborated by Albert Bandura and Richard Walters. These authors propose that regardless of frustration, suitable cues will lead to aggressive responses as a result of internalization of successful aggressive models or direct reinforcement of aggressive behavior, and they cite evidence for the proposition that direct reinforcement and modeling processes produce aggressive behavior even at low levels of frustration.[25] The master assumption of the social learning explanation of potential for political violence is that experiencing goal attainment as the result of political violence (direct reinforcement), or perception that others with whom one identifies have achieved goal attainment by means of political violence (modeling), produces a readiness to engage in political violence.

Although his aggregate data did not support the contention, Gurr has suggested that "if we have shared discontents, our attitudes about the legitimacy of our rulers and political institutions either focus that discontent on or deflect it away from political targets."[26] This line of reasoning seems sensible enough: legitimacy sentiment is proposed to serve as a politicizing agent, providing a rationale either for blaming the regime for relative deprivation or for deflecting blame onto other institutions in the society. Additionally, Bandura and Walters have argued that "the over-all results from studies of direct reinforcement and modeling of aggression call for important revisions in the frustration-aggression hypothesis . . . the nature of the response to frustration will depend on he prior social training of the frustrated subject, or, more specifically, on the reinforcement and modeling procedures which he has previously experienced."[27] In accord with this line of reasoning, belief that political violence has led to goal attainment may be proposed as a factor that serves to disinhibit or inhibit the tendency for relative deprivation to lead to potential for political violence.

Other factors that presumably help account for potential for political violence include satisfaction-dissatisfaction with the immediate performance of incumbent political authorities; generalized expectancy of mastery-powerlessness over the locus of control of reinforcement in one's personal life and in political influence attempts; and degree of isolation from community institutions.[28] A comprehensive theory might include all of these concepts. At this preliminary stage, however, I shall concentrate only on relative deprivation, the most commonly invoked concept, and on two other concepts that seem central to the explanation of potential for political violence: belief that political violence has led to goal attainment, and diffuse support for the political authority structure.[29]

This partial theory of potential for political violence may be stated in propositional form as follows:

(1) Potential for political violence will vary directly with belief that political violence has led to goal attainment, regardless of diffuse support for the political authority structure and regardless of relative deprivation.

(2) Potential for political violence will vary inversely with diffuse support for the political authority structure, regardless of belief that political violence has led to goal attainment and regardless of relative deprivation.

(3) Potential for political violence will not vary with relative deprivation, regardless of belief that political violence has led to goal attainment and regardless of

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[23] Muller, "Correlates and Consequences . . . ," pp. 08–409.

[24] See Table 8 at page 1213 in Joel L. Aberbach and ack E. Walker, "Political Trust and Racial Ideology," *American Political Science Review*, 64 (December, 970), 1199–1219.

[25] Albert Bandura and Richard H. Walters, *Social Learning and Personality Development* (New York: Holt, Rinehart and Winston, 1963), esp. ch. 3.

[26] Gurr, *Why Men Rebel*, p. 193.

[27] *Bandura and Walters*, p. 159.

[28] Powerlessness and isolation, along with racial dissatisfaction, have been shown to correlate with willingness to use violent methods for goal attainment, among blacks, in H. Edward Ransford, "Isolation, Powerlessness, and Violence: A Study of Attitudes and Participation in the Watts Riot," *American Journal of Sociology*, 73 (March, 1968), 581–591.

[29] In *Sources of Potential for Political Violence: A Test of a Partial Theory* (unpublished Ph.D. dissertation, University of Iowa, 1971), from which portions of this report derive, I proposed a hierarchical multistage model. This report focusses on a single-stage model which avoids debatable assumptions about the sequential ordering of terms.

diffuse support for the political authority structure.

One implication of these propositions is that a high level of relative deprivation is not a necessary condition for a high level of potential for political violence. Another implication is that, if potential for political violence is observed to vary directly with relative deprivation, this is because diffuse support for the political authority structure and/or belief that political violence has led to goal attainment vary inversely and directly with relative deprivation, respectively. Finally, potential for political violence will vary more strongly with both diffuse support for the political authority structure and belief that political violence has led to goal attainment combined, than with either of these factors separately.

The first and second sections of this report will discuss the selection of the subjects for the study and the formation of the operational indicators of the concepts in propositions 1–3. In the third section, a model of potential for political violence suggested by propositions 1–3 will be tested.

Research Design

A number of explanations of civil strife/political violence have been subjected to empirical test using cross-national aggregate data.[30] But nations do not take periodic inventories of many of the independent variables critical to these explanations. A recent criticism of the work of Gurr—that he "faces serious difficulty in converting available quantitative data into convincing indicators of the theoretical variables he hopes to relate to violence"—is applicable to many of the extant violence studies utilizing aggregate data.[31]

Survey research is one route to take for testing explanations of political violence that refer to micro-level processes. Some interesting survey work has been done, although a number of the studies seem to have been designed as much to collect descriptive generalizations about black residents of urban areas in the United States as to test semiuniversal or universal generalizations that might be part of a theory o potential for political violence.[32]

The data base here is a survey of residents o Waterloo, Iowa. The fourth largest city i Iowa, Waterloo has about 78,000 residents o which somewhat more than 8 per cent ar black. The Kerner report on civil disturbance classified Waterloo as having experienced "serious" disorders in 1967.[33] Since that time th Iowa National Guard had to be called in during civil disturbances in September, 1968, an racial tension has been increasing. A repor from the School of Journalism and Communications at the University of Iowa cited Waterloo as a prototype city: "it earned its nickname 'Little Chicago' in part because it shares the urban problems of that city and hundreds more like it across the country."[34] Waterloo, on a small scale, has the standard physical trademarks of the modern American metropolitan setting: decaying inner city and black ghetto bounded by well-trimmed, sometimes plush, lily-white residential areas. Bernard D. Nossiter, a journalist for the *Washington Post,* has described Waterloo this way:

> Criss-crossed by Illinois Central Railroad tracks, its downtown a jumble of squat stone structures and a few characterless modern office boxes, its roads pockmarked with pizza emporiums, franchised restaurants, gas stations and motels, commercial Waterloo is not a pretty sight. But its well-tended homes, small parks and trees speak of an agreeable life.[35]

Needless to say, "its well-tended homes, small parks and trees . . ." refers mainly to white residential Waterloo; the blacks are almost wholly confined to a ghetto in the north end of town.

Waterloo was chosen as a research site for several reasons: budgetary constraints dictated that the interviewing be conducted in a single Midwestern city; for substantive purposes it was important to select a city in which political violence was a salient phenomenon, so as to observe variations sufficient for complex multivariate analysis; and also, of course, the city had to be reasonably representative of other urban communities in the United States. Waterloo seemed to fit this bill of particulars as well as any other candidate in the Midwest.

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[30] Cf. Davies, "The J-Curve of Rising and Declining Satisfaction . . . ," Feierabend et al., "Social Change and Political Violence . . . ," Gurr, "A Causal Model of Civil Strife . . . ," Seymour Spilerman, "The Causes of Racial Disturbances: A Comparison of Alternative Explanations," *American Sociological Review,* 35 (August, 1970), 627–649.

[31] William Flanagan and Edwin Fogelman, review of *Violence in America: Historical and Comparative Perspectives,* ed. Graham and Gurr in the *American Political Science Review,* 64 (March, 1970), 192.

[32] One exception is the report by Aberbach and Walker, "Political Trust and Racial Ideology," in which a general explanation of Political Trust is tested across both blacks and whites and the relationship between Political Trust and extreme political behavior is also tested across both races. Also, an interesting report by Lester Milbrath, "People and Government," Department of Political Science, State University of New York at Buffalo, mimeo, 1971, received too late to be mentioned in this paper, tests a number of general hypotheses about correlates of protest behavior across both races.

[33] Waterloo also was ranked at level 2 on Wanderer's Guttman scale of riot severity which ranges from a low of "8" to a high of "1"; reported in Jules J. Wanderer, "An Index of Riot Severity and Some Correlates," *American Journal of Sociology,* 74 (March, 1969), 500–505.

[34] "Waterloo: Study of a Social Crisis," supplement to *The Daily Iowan,* May 23, 1969, p. 1.

[35] Bernard D. Nossiter, "An Outsider Takes a Look at Waterloo," *Des Moines Sunday Register,* May 24, 1970 (copyright, *The Washington Post*), p. 4–G.

Two samples were drawn from the city of Waterloo. The Survey division of the Statistical Laboratory at Iowa State University drew the Citizen sample and conducted the fieldwork for it between March and May, 1970. A Community Leaders sample was drawn from a series of questions in the Citizen interview schedule designed to identify influential members of the group infrastructure in Waterloo. Personnel trained by the Iowa State Statistical Laboratory conducted the interviewing of Community Leaders in June and July, 1970. The Community Leaders sample contains 115 respondents. Identical interview schedules were administered to both samples, thus affording useful mass-elite comparisons for future analysis.

The present focus of inquiry, the Citizen sample, is a disproportionately stratified, multistage random probability—or cluster—sample.[36] The universe consisted of all household residents of Waterloo 18 years old and older. Since the main concern of this project is explanation and prediction rather than description, a "scope" sample was desired.[37] In brief, a scope sample is simply one in which the investigator tries to exercise some control over the variation on certain variables which otherwise might be highly skewed, hopefully ensuring variation sufficient for adequate empirical testing of hypothesis. In this instance, blacks were sampled at a higher rate than whites.[38] Although blacks make up only 8 per cent of the Waterloo population, they constitute 38 per cent of the Citizen sample. Presumably, this procedure has the effect of decreasing the skewness of the distribution of responses to many of the variables, relative to that which would result from a straightforward unstratified random probability sample consisting overwhelmingly of whites. Thus, multivariate analysis and testing of hypothesis about relationships across the full range of values of relevant variables is facilitated. On the other hand, since random sampling methods were used *within* strata, it is also possible to describe the distributions on the variables among blacks and whites separately for the Waterloo population circa 1970.

From among the 626 individuals designated, 503 (or 80 per cent) completions were obtained. Within strata the completion rate of 80 per cent (generally accepted as a lower limit) is unaltered: of 241 blacks designated, 193 completions were obtained; of 375 whites designated, 310 completions were obtained. For the most part, black interviewers interviewed blacks, and white interviewers interviewed whites.

Operational Definitions of the Variables

In this section, operational indicators of the concepts in propositions 1–3 will be defined. Hypotheses about the zero-order association between each psychological variable and a Potential for Political Violence scale will be tested. Also, the effect of two surrogate variables, race and age, will be taken into account.

Potential for Political Violence. If a citizen disagreed with actions of his government and desired to see such actions changed, he might engage in a variety of behaviors. Some behaviors would conform to the legal norms of the regime, others would deviate from the legal norms; some behaviors would conform to the customary norms of the regime, others would deviate from the customary norms.[39] Behaviors that conform to legal and customary regime norms—at least in most polyarchies—constitute the domain of Lester Milbrath's Hierarchy of Political Involvement.[40] A peaceful demonstration which complied with all relevant regulations is an example of a legal and noncustomary behavior. Illegal and noncustomary behavior ranges from passive civil disobedience to violent revolution.

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[36] Within Waterloo, segments (size 3 housing units) of city blocks were selected; dwelling units were then randomly selected within each segment and designated respondents within the dwelling units were interviewed. No substitution was allowed.

[37] On scope sampling see David Willer, *Scientific Sociology* (Englewood Cliffs: Prentice-Hall, 1967), pp. 97–115.

[38] On the basis of a report entitled "Neighborhood Analysis" (an update of the 1960 census) conducted by the Waterloo City Planning Commission in 1967, the area of the city containing almost all the black residents was identified and sampled at a higher rate than the area containing almost all the white residents. Within each area, male-female response rates differed slightly. The response rates were, by type of area and sex: "Black," male = .79; "Black," female = .83; "White," male = .75; "White," female = .84.

[39] Where "norms" are rules specifying the way members of a political system are expected to participate in the political process; "legal norms" are formal prescriptions, binding upon all members, whose violation may be met with official sanction or punishment; "customary norms" are widely held beliefs about right and wrong ways of participating in the political process. For a more extensive discussion of these terms see David Easton, *A Systems Analysis of Political Life*, pp. 190–211.

[40] See Figure 3 in Lester Milbrath, *Political Participation* (Chicago: Rand McNally, 1965), p. 18.

Note that I have been differentiating political dissent behavior on the basis of the presence or absence of three properties: (1) conformity to legal regime norms, (2) conformity to customary regime norms, (3) the use of violence. I shall now define the concept "potential for political violence" as degree of readiness to support (by virtue of belief and willingness to engage in) violent political dissent behavior which deviates from legal and customary regime norms. Degree of readiness is a continuum ranging from a low end, defined by intention to avoid all forms of political behavior which deviate from legal and customary regime norms regarding such participation, to a high end, defined by intention to engage in behavior which deviates from legal and customary regime norms and involves the use of violence against the state.

Figure 1 depicts a model of the potential for political violence concept. The model is monotone deterministic; it represents a perfect "Guttman scale." The ordinate shows the probability of approving or expressing an intention to engage in a particular dissent behavior. Values of an "approve political violence" and "intention to engage in political violence" continuum are shown along the abscissa. The trace lines show the predicted probability of a positive response to each of five different dissent behaviors which are proposed to discriminate perfectly at particular intervals on the abscissa.

Each behavior embodies a combination of properties; these behaviors are rank-ordered with respect to the degree of challenge they pose to a political regime. Level "5" involves the planned use of violence to overthrow the regime; level "4" involves relatively unplanned and sporadic violence to attack the regime; level "3" involves nonviolent but disruptive deviance from legal regime norms; level "2" involves nonviolent and generally nondisruptive deviance from legal regime norms; level "1" involves conformity with legal regime norms and nonviolent deviance from customary regime norms; level "0" involves avoidance of any deviation from customary and legal regime norms and use of violence.

The full wording of the items designed to discriminate among different levels of potential for political violence is reported in the Appendix. Respondents were asked to report (1) their approval-disapproval of each dissent behavior and (2) their willingness, under present circumstances, to engage in each behavior. For this sample the items show a satisfactory degree of fit to the cumulative model: The coefficient of reproducibility (CR) for the "approval" item-pool is .91; CR for the "intention to engage" item-pool is .93.[41]

Table 1 reports the distribution, by race, on the Approval of Political Violence (APV) and Intention to Engage in Political Violence (IEPV) scales. As one might anticipate, blacks are more likely to rank high on Approval of Political Violence than whites. Only 11 per cent of the blacks do not approve any deviant dissent behavior, whereas 26 per cent of the whites do not approve any deviant dissent; and the percentage of whites approving only of deviance from customary regime norms is almost twice as great as the percentage of blacks at this position. These levels ("0" and "1") constitute "low" APV; they indicate rejection of any dissent behavior that does not conform to the official "rules of the game," thus entailing overt support for the existing structure of political authority. By contrast, levels "2" and "3" ("medium" APV) comprise behavior which does challenge the existing structure of political authority, although it does not involve the use of violence in actually attacking the existing structure. Blacks are more likely to show "medium" APV than whites. Levels "4" and "5" constitute "high" APV; they comprise behavior which, because it involves the use of violence, poses a potentially serious threat to the existing structure of political authority. More blacks than whites score at these "high" levels of APV; almost one-tenth of the blacks approve dissent behavior up to armed revolution, whereas only one one-hundredth of the whites approve dissent behavior to this extreme.

Similarly, blacks are more likely to rank high on Intention to Engage in Political Violence than whites. Considerably fewer blacks score at the lowest level of IEPV than whites. At medium and high IEPV levels, blacks are consistently more disposed to engage in dissent behaviors which present progressively greater challenges to the existing structure of political authority; but it is apparent that, among both groups, more respondents approve such behaviors than will engage in them. At the level of violent revolution, only 4 per cent of the blacks report that they would engage in this behavior

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[41] For the Community Leaders sample, CR is .95 and .94, respectively. See Edward N. Muller, "Measurement of Readiness for Unconventional Political Participation," Department of Political Science, State University of New York at Stony Brook, ditto, 1972.

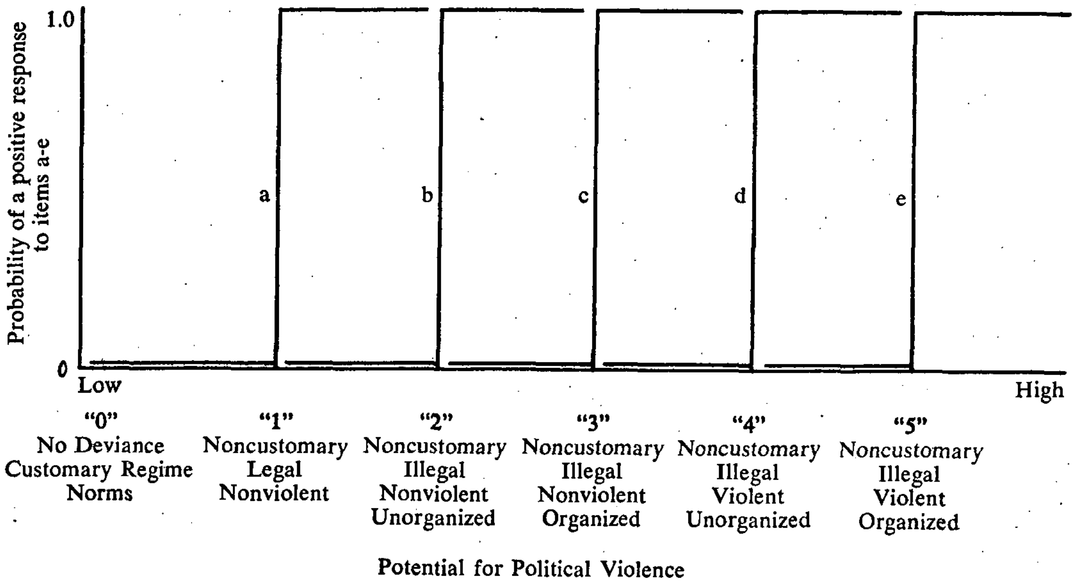

Figure 1. Hypothetical Trace Lines for Items that Discriminate Perfectly at Various Levels of Potential for Political Violence

plus all the others—down from the 9 per cent who approve this level and all the others; whites draw the line at the violent demonstration and property destruction level: none of them also would engage in violent revolution.

How do we interpret the potential for political violence in Waterloo? On the one hand, neither blacks nor whites are about to present a serious imminent challenge to the state by revolution. However, there is perhaps more potential for revolutionary behavior among blacks than some people might expect—particularly those who interpret the riots as "senseless behavior."

It is clear from the distributions shown in Table 1 that moral support for antiregime dissent and willingness to engage in antiregime dissent are not perfectly correlated in this sample. If these scales have validity, one would expect that no respondents would rank higher on IEPV than on APV—would engage in a level of dissent behavior of which they do not approve. One can conceive, however, of a number of reasons why men who approve of a given level of dissent might nevertheless refuse to engage in that level of dissent. This discrepancy is apparent from responses to the open-ended items included in the question-sets designed to

Table 1. Percentage Approval of and Intention to Engage in Political Violence, by Race

Approval of Political Violence (APV)	Scale score	Blacks	Whites	Intention to Engage in Political Violence (IEPV)	Scale score	Blacks	Whites
Disapprove dissent	0	11	26	Not engage dissent	0	34	58
Only approve legal demonstrations	1	22	41	Only engage legal demonstrations	1	29	30
And approve disobey unjust law	2	34	22	And engage disobey unjust law	2	20	08
And sit-ins, demos, bldg take-overs, etc	3	16	06	And sit-ins, demos, bldg take-overs, etc	3	10	03
And violent actions against police, etc	4	08	05	And violent actions against police, etc	4	03	01
And arm for battle with gov't forces	5	09	01	And arm for battle with gov't forces	5	04	00
		100%	101%[a]			100%	100%

[a] Rounding error.

measure potential for political violence. (See Appendix for the format of the question-sets.) Among respondents who *approve* of a given level of dissent, but would not *engage* in the corresponding level of dissent, two factors (from a preliminary scanning of the open-ended responses) appear to stand out as possible causes. Age is an implicit or explicit deterrent among elderly respondents, and fear of reprisal from the state is another deterrent.

I shall hypothesize that (1) APV and IEPV will show a strong degree of fit to a direct monotonic function[42] and (2) cases which depart from direct monotonicity will show higher rank-order on APV than on IEPV. Table 2 shows that both hypotheses are supported. Somer's *d* (symmetric) is .53: that is, 53 per cent of the total ordinal variation in APV and IEPV is shared; assuming interval measurement, r^2 is .47. Also, out of the entire sample, only one lone respondent is considerably *lower* on Approval of Political Violence than on Intention to Engage in Political Violence. But quite a few are considerably *higher* on Approval of Political Violence than on Intention to Engage in Political Violence. (To facilitate interpretation, the diagonal on the bivariate frequency distribution has been bounded by broken lines and the frequencies which fall directly adjacent to the diagonal bounded by solid lines.) Future analysis, using the open-ended reponses, may account for many of these latter discrepancies, particularly in terms of respondents' age and perception of government coercive potential. At any rate, most of the cases do fall, as expected, on or directly adjacent to the diagonal.

Table 2. Intention to Engage in Political Violence and Approval of Political Violence

Intention to Engage in Political Violence	Approval of Political Violence						
	0	1	2	3	4	5	N=
0	97	89	43	9	2	2	242
1	5	75	45	17	4	2	148
2	0	5	39	8	11	2	65
3	0	0	3	13	8	4	28
4	0	0	1	1	3	4	9
5	0	0	0	0	2	5	7
N=	102	169	131	48	30	19	499

$d_{sym} = .53$ $r^2 = .47$

Potential for Political Violence, the dependent variable in this report, is a summation of scores on APV and IEPV—a linear combination of willingness to give moral support for, and to engage in, the behavior ranged along the political violence continuum. The correlation (r) between Intention to Engage in Political Violence and Potential for Political Violence is .90; the correlation (r) between Approval of Political Violence and Potential for Political Violence is .93.

Relative Deprivation. Among citizens in general, welfare values are by far the most salient class: they accounted for approximately 80 per cent of all the personal value concerns among 863 million people in 12 nations, according to a study directed by Hadley Cantril.[43] Within this class, four categories of value concerns were selected as referents for the measures of relative deprivation: career satisfaction, economic well-being, satisfactoriness of living conditions, children's welfare.

The Cantril scale was used to define perceived achievement optimum-achievement (AO-A) discrepancy with respect to each of the four welfare values at three different points in time: past, present, future. A composite measure of short-term relative deprivation is defined by AO-A discrepancies in the present; it has been labeled Short-Term Welfare Gratification, since the referent is welfare values and the high end of the scale represents complete AO-A congruence, while the low end represents complete AO-A incongruence. A composite measure of long-term relative deprivation is defined by an equation which adjusts present AO-A discrepancy for the rate of change in AO-A discrepancy from past to future; it has been labeled Long-Term Welfare Gratification. (See the Appendix for a full discussion of the wording of the items and the rationale and procedures used for construction of the composite relative deprivation scales.)

Table 3 shows the average gratification-deprivation scores—on a scale ranging from 10 to 0—for blacks and whites on each welfare value. Past, present, and future discrepancy perceptions by both blacks and whites are consistently different, as shown by the fact that every F-value is statistically significant at less than the .001 level. Perhaps the most striking aspect of these analyses of variance results is the direction of the average scores on the welfare values. For every welfare value, whites *and blacks* consistently perceive *decreasing* AO-A discrepancy over time; that is, they consistently perceive a narrowing gap between achievement optimum and achievement (as defined by the Cantril scale). Among whites, this perception of positive change is not surprising. But among blacks, it is the reverse of what would be expected if credence could be placed in James C. Davies's contention that measures of objective deprivation demonstrate that AO-A discrepancies began *increasing* among blacks around 1963, and have been steadily on the upswing ever since, therefore causing the riots, etc. of the late 1960s.[44]

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[42] By "strong degree of fit" I mean at least 20 per cent of the variance accounted for. Throughout this paper, then, a "strong" correlation refers to r ≥ .45.

[43] For the original data see Hadley Cantril, *The Pattern of Human Concerns* (New Brunswick: Rutgers University Press, 1965). This estimate is based on Table 3 in Gurr, *Why Men Rebel*, p. 70. I have included the 16 per cent of the interpersonal concerns which relate to the family as part of the Self-Actualization component of Welfare values.

One might be tempted to write off the above finding as an anomaly due to some peculiarity of the situation of blacks in Waterloo, if it were not that previous studies conducted in Buffalo and Detroit found that blacks in these two cities also perceived decreasing AO-A discrepancy over time—in this case, with respect to their personal value concerns as a whole.[45] These Waterloo data, coinciding as they do with the earlier studies, clearly suggest revision of notions about widespread perception of increasing AO-A discrepancy among blacks. In fact, on the measure of Children's Education Gratification, the average black sees himself as achieving, in the next five years, a level that is practically identical to that expected by the average white.

Of course average levels of relative gratification-deprivation provide no information about the *relationship* between level of gratification-

Table 3. Mean Welfare Gratification-Deprivation Over Time

Mean Work Gratification Scores:	Past	Present	Future		Mean Square	F	
Blacks	5.12	5.57	6.51	Between	83.15	9.02	$p<.001$
N=	(175)	(181)	(158)	Within	9.22		
Whites	6.32	6.85	7.46	Between	80.15	10.63	$p<.001$
N=	(250)	(277)	(238)	within	7.54		
Mean Income Gratification Scores:	Past	Present	Future		Mean Square	F	
Blacks	5.07	5.18	6.21	Between	64.74	7.76	$p<.001$
N=	(175)	(180)	(160)	Within	8.35		
Whites	5.75	6.53	7.13	Between	135.91	21.84	$p<.001$
N=	(283)	(306)	(278)	Within	6.22		
Mean Housing Gratification Scores:	Past	Present	Future		Mean Square	F	
Blacks	5.16	5.49	6.67	Between	108.49	13.94	$p<.001$
N=	(177)	(182)	(169)	Within	7.78		
Whites	6.60	6.70	7.41	Between	57.95	8.88	$p<.001$
N=	(289)	(308)	(293)	Within	6.53		
Mean Children's Education Gratification Scores:	Past	Present	Future		Mean Square	F	
Blacks	5.60	6.90	7.84	Between	161.76	22.87	$p<.001$
N=	(126)	(135)	(128)	Within	7.07		
Whites	6.45	7.25	7.86	Between	73.72	12.30	$p<.001$
N=	(140)	(163)	(157)	Within	5.99		

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[44] See Davies, "The J-Curve of Rising and Declining Satisfactions . . . ," pp. 700–702.

[45] See Table 1 in Everett F. Cataldo, Richard M. Johnson, and Lyman A. Kellstadt, "Social Strain and Urban Violence," in *Riots and Rebellion* . . . , ed. Masotti and Bowen, p. 287; also see footnote 50 in Aberbach and Walker, "Political Trust and Racial Ideology," p. 1208.

Table 4. Correlations (r) Between Short-Term and Long-Term Welfare Gratification and Potential for Political Violence

	LWG	PPV
Short-term Welfare Gratification (SWG)	.626*	−.234*
Long-term Welfare Gratification (LWG)		−.010†

* $p < .001$.
† $p > .05$.

Table 5. Percentage Support for Elements of the Legitimating Ideology of the Regime, by Race

Propositions		Blacks Adjusted Percent*	Whites Adjusted Percent*
The national government should attempt to break up large business corporations.	Agree	21	33
	Neutral	13	13
	Disagree	66	53
	N=	(175)	(299)
In the U.S. personal success is attainable for anyone who wants it, by hard work and skill.	Agree	65	84
	Neutral	03	02
	Disagree	32	14
	N=	(186)	(308)
Democracy is the best form of government.	Agree	78	90
	Neutral	14	07
	Disagree	09	03
	N=	(174)	(305)
There are times when it almost seems better for the people to take the law into their own hands rather than wait for the machinery of government to act.	Agree	23	19
	Neutral	16	04
	Disagree	62	77
	N=	(181)	(308)
Minorities should always be allowed to criticize the majority.	Agree	46	58
	Neutral	15	11
	Disagree	40	31
	N=	(171)	(307)
The Constitution of the U.S. is in need of major change.	Agree	41	40
	Neutral	22	11
	Disagree	36	49
	N=	(174)	(302)

* Don't Know responses excluded.

deprivation and level of potential for political violence. It could easily be the case that, even though AO-A discrepancy is perceived to be decreasing on the average, high discrepancy is associated with high potential for political violence and low discrepancy is associated with low potential for political violence. The relative deprivation thesis suggests the following hypotheses:

PPV_1: Potential for Political Violence will be strongly and inversely correlated with Short-Term Welfare Gratification.

PPV_2: Potential for Political Violence will be strongly and inversely correlated with Long-Term Welfare Gratification.

Table 4 shows that these hypotheses are not supported. PPV is weakly correlated with SWG ($r = -.234$); furthermore, the $-.010$ correlation between PPV and LWG indicates that there is no linear relationship at all between these variables. Since PPV shows no tendency to vary inversely with LWG, only the SWG term will be used to measure relative deprivation in testing the propositions of the potential for political violence model.

Diffuse Support for the Political Authority Structure. The measures of ideological, structural, and personal dimensions of legitimacy used here are much the same as those developed in the pilot study. The earlier study suggested some minor changes in the wording of the items used to measure structural legitimacy. In the present study an additional item was added to the Legitimating Ideology measure so as to increase sampling adequacy—"Minorities should always be allowed to publicly criticize the majority."[46] Attention also was paid to improving the sampling adequacy of the operational indicator of the personal dimension of legitimacy.

As the distributions in Table 5 show, blacks and whites generally express considerably greater positive than negative support for the propositions selected as representative of the legitimating ideology. Blacks express less positive support for these propositions than whites, but the differences are of relatively slim magnitude. Both races show the greatest positive support for the general proposition, "Democracy is the best form of government." Its more specific counterpart, however, "Minorities should always be allowed to publicly criticize the majority," is one of the two propositions that elicit comparatively weak positive support. This finding accords with the well established proposition that, among mass publics, while agreement on abstract regime norms may be high, agreement on specific applications of abstract principles will be considerably lower.[47]

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[46] The legitimating ideology items were designed to measure five values which Robert A. Dahl has identified as important components of the American legitimating ideology. See the discussion in "Correlates and Consequences . . . ," p. 398 and Robert A. Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966), pp. 35–43 and p. 63.

An interesting finding is that blacks are more supportive than whites of the private property regime norm, as measured by the item, "The national government should attempt to break up large business corporations." If this finding is not just peculiar to this sample, it suggests that anti-regime ideology based on a Marxist framework will have little appeal to blacks. Another finding of interest is the low support for the Constitution among both blacks and whites, but especially among blacks. Whether this represents desires for fundamental change, desires for certain specific moderations, or a general diffuse dissatisfaction—perhaps with the manner in which the Constitution has been interpreted—cannot be determined, since an open-ended response item was not included.

Because stable beliefs about the values contained in the legitimating ideology require rather high degrees of cognitive and moral development, and because these beliefs are learned only in adolescence,[48] one might expect to find a fairly low degree of psychological constraint among these beliefs. This is the case. They are not consistently interrelated and, when subjected to a principal component factor solution, they split distinctly apart rather than loading on a single dimension. The Legitimating Ideology scale makes no pretense of tapping an underlying attitude or belief syndrome. As a *coherent system* of ethical principles and applications of these principles, the legitimating ideology of the regime is not meaningful to most citizens. The Legitimating Ideology scale is a summation of scores on these items; but it measures only the *number* of regime values with which a person agrees or disagrees. The assumption is that straightforward support for regime values, regardless of internal consistency, is a dimension of legitimacy that might be of some importance for behavioral intentions.[49]

Figures 2 and 3 show the distribution by race, on the items designed to measure attachment to the institutional framework of the regime—structural legitimacy. Like beliefs in regime values, beliefs in the legitimacy of regime structures are learned relatively late and presumably require a relatively high degree of cog-

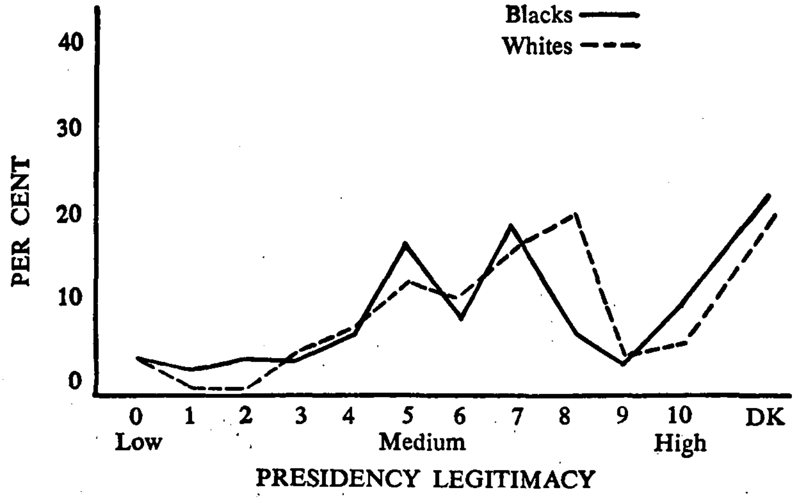

Figure 2. Scope of Diffuse Support for Regime Structures—The Presidency, by Race

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[47] Cf. James W. Prothro and Charles M. Grigg, "Fundamental Principles of Democracy: Bases of Agreement and Disagreement," *Journal of Politics,* 22 (May, 1960), pp. 276–294; Herbert McClosky, "Consensus and Ideology in American Politics," *American Political Science Review,* 58 (June, 1964), 361–382.

[48] On the acquisition of affect for the norms of the regime see pages 238 to 245, David Easton and Robert L. Hess, "The Child's Political World," *Midwest Journal of Political Science,* 6 (August, 1962), 229–246.

[49] For the student sample the correlation between Legitimating Ideology and Avoid Protest Action was .348; see Table 2 in "Correlates and Consequences . . . ," p. 402.

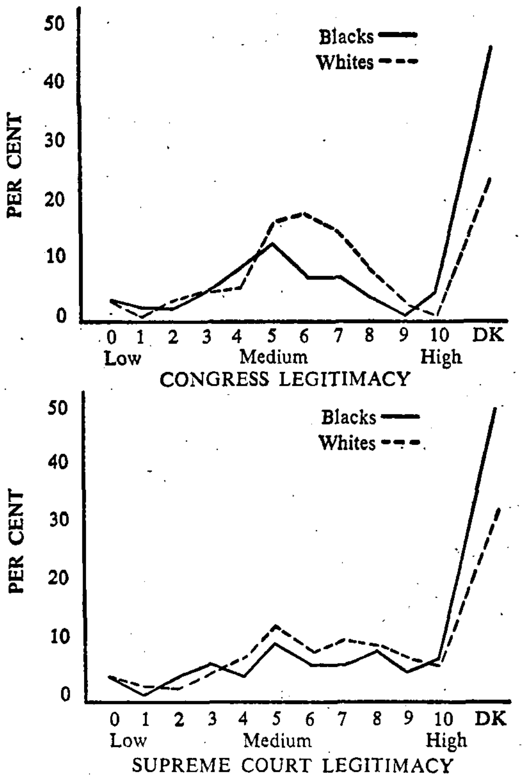

Figure 3. Scope of Diffuse Support for Regime Structures—Congress and the Supreme Court, by Race

nitive and moral development. Not surprisingly then, on Presidency Legitimacy, the "DK" category is the second highest peak of the white distribution, characterizing 19 per cent of that population; and the "DK" category is the highest peak on the black distribution, containing 19 per cent of that population. Affect toward the Presidency as an institution tends to be distributed slightly toward the positive or high end of the continuum for both blacks and whites, but there also is considerable negative affect.

Among both races, the "DK" category is the highest peak of the distribution on Congress Legitimacy and Supreme Court Legitimacy. Congress as a regime institution (an institutional framework distinct from its members) is not salient to a full 47 per cent of the blacks; to 49 per cent, the Supreme Court as a regime institution is not salient.[50] The corresponding proportions for nonsalience of the Congress and the Court among whites are 25 and 32 per cent. Moreover, the openended responses to the structural legitimacy items indicate that even among those who do express opinions, some tend to equate the institution itself with its particular occupants. (The wording of the structural legitimacy items is fully discussed in the Appendix.)

Diffuse support for the President as a person is apparently the earliest belief with political content that children in the United States acquire.[51] Therefore, among adults, the Presidency should be a more salient institution than Congress and the Supreme Court. These data support that prediction.

Support for these institutions is lower in this mass sample than was found to be the case for the student sample analyzed earlier.[52] Blacks tend to show only moderate support for Congress as an institution; Whites appear slightly more supportive. Both races also tend to show moderate support for the Supreme Court as an institution. This contrasts sharply with the high support for these institutions manifested by the student sample. The high support shown by the students could reflect the fact that, as their open-ended responses show, they generally can distinguish between structures per se and the behavior of the occupants of such structures at particular times.

The acquisition of affect toward relatively abstruse political objects like regime structures is presumably correlated with level of formal education attained. Level of education will be a surrogate in part for level of cognitive and moral development and in part for exposure to "civics training" and more substantive academic courses that might stimulate the acquisition of affect toward regime structures.[53]

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[50] On a question which inspired the measures of structural legitimacy used here, Walter F. Murphy and Joseph Tanenhaus found that 34.7 per cent of a 1966 national sample of Americans had no idea what the main job of the Court is: see Walter F. Murphy and Joseph Tanenhaus, "Public Opinion and the United States Supreme Court: A Preliminary Mapping of Some Prerequisites for Court Legitimation of Regime Changes," in *Frontiers of Judicial Research*, ed. Joel B. Grossman and Joseph Tanenhaus (New York: Wiley, 1969), p. 282. If the proportion across the total sample here (weighted by race) for whom the Court is not visible were computed it would look about the same as that in the national sample.

[51] Cf. David Easton and Jack Dennis, "The Child's Image of Government," *The Annals*, 361 (September, 1965), pp. 40–57; Fred I. Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965); Robert D. Hess and Judith V. Torney, *The Development of Political Attitudes in Children* (Chicago: Aldine, 1967); Easton and Dennis, *Children in the Political System.*

[52] See Table 1 in Muller, "Correlates and Consequences . . . ," p. 396.

[53] The importance, however, of formal "civics training" itself may be slight. See pages 865–867 of Kenneth P. Langton and M. Kent Jennings, "Political So-

Table 6 shows the proportion of nonresponse and response to the structural legitimacy items by level of formal education and by race. Among both blacks and whites, college-educated respondents are far more likely to express affect toward the Congress and Supreme Court as regime institutions than are respondents whose formal education did not progress beyond the grade-school level; the college-educated also are more likely to express affect toward the Presidency as a regime institution than the grade-school educated, but the percentage differences are not so pronounced.

Expression of affect toward Congress is a direct monotonic function of level of education among blacks and whites; but on expression of affect toward the Supreme Court and Presidency, an interesting difference appears between the races. Affect toward these institutions is a direct monotonic function of level of education only among whites; blacks with a college education are somewhat more likely to express affect than those with a high-school or grade-school education, but the differences between these proportions are not particularly large, and the differences between the high-school and grade-school groups are very small.

Does level of diffuse support for these institutions increase as level of education increases? Table 7 shows the proportion of respondents at low, medium, and high levels of structural legitimacy by level of education and race. For both races, neither Congress Legitimacy nor Presidency Legitimacy show a consistent tendency to increase as level of education increases. However, there is a somewhat sizeable contrast between college-educated blacks and non-college-educated blacks in diffuse support for the Presidency: No college-educated blacks are low on Presidency Legitimacy, as opposed to 15 per cent of high-school and grade-school blacks who rank low on this variable; 75 per cent of the college-educated blacks are high on Presidency Legitimacy, as opposed to 45 per cent of high-school and 48 per cent of grade-school blacks. Either the high diffuse support for Congress manifested by the student sample is atypical of college students, or postcollege so-

Table 6. Percentage Nonresponse-Response to Structural Legitimacy Items by Level of Education and Race[a]

Legitimacy Congress	Blacks Education				Whites Education			
	Grade	High	College	(N)	Grade	High	College	(N)
Nonresponse	55	44	22	(91)	42	26	06	(76)
Response	45	56	78	(102)	58	74	94	(234)
(N)	100%	100%	100%		100%	100%	100%	
	(74)	(110)	(9)		(59)	(182)	(69)	

Legitimacy Supreme Court	Blacks Education				Whites Education			
	Grade	High	College	(N)	Grade	High	College	(N)
Nonresponse	47	52	33	(95)	53	36	03	(99)
Response	53	48	67	(98)	47	64	97	(211)
	100%	100%	100%		100%	100%	100%	
(N)	(74)	(110)	(9)		(59)	(182)	(69)	

Legitimacy Presidency	Blacks Education				Whites Education			
	Grade	High	College	(N)	Grade	High	College	(N)
Nonresponse	20	25	11	(43)	32	20	03	(58)
Response	80	75	89	(150)	68	80	97	(252)
	100%	100%	100%		100%	100%	100%	
(N)	(74)	(110)	(9)		(59)	(182)	(69)	

[a] Education: up to completed eighth grade = grade; some high school to completed high school plus technical school = high; some college to completed college plus graduate school = college.

Table 7. Percentage Belief in Structural Legitimacy by Level of Education and Race[a]

Legitimacy Congress	Blacks Education				Whites Education			
	Grade	High	College	(N)	Grade	High	College	(N)
Low	21	21	00	(20)	12	11	17	(30)
Medium	49	50	86	(53)	50	54	46	(120)
High	30	29	14	(29)	38	35	37	(84)
	100%	100%	100%		100%	100%	100%	
(N)	(33)	(62)	(7)		(34)	(135)	(65)	

Legitimacy Supreme Court	Blacks Education				Whites Education			
	Grade	High	College	(N)	Grade	High	College	(N)
Low	26	21	00	(21)	00	16	19	(31)
Medium	36	34	17	(33)	25	43	40	(84)
High	39	45	83	(44)	75	41	40	(96)
	101%[b]	100%	100%		100%	100%	99%[b]	
(N)	(39)	(53)	(6)		(28)	(116)	(67)	

Legitimacy Presidency	Blacks Education				Whites Education			
	Grade	High	College	(N)	Grade	High	College	(N)
Low	15	15	00	(21)	18	06	13	(25)
Medium	37	41	25	(58)	45	34	30	(87)
High	48	45	75	(71)	38	60	57	(140)
	100%	101%[b]	100%		101%[b]	100%	100%	
(N)	(59)	(83)	(8)		(40)	(145)	(67)	

[a] Structural legitimacy items: 0–3 = low; 4–6 = medium; 7–10 = high.

Education: up to completed eighth grade = grade; some high school to completed high school plus technical school = high; some college to completed college plus graduate school = college.

[b] Rounding error.

cialization experiences reduce high diffuse support for Congress acquired during the college years. The finding that diffuse support for the national legislative institution does not vary monotonically with education for this sample contrasts with the finding reported by G. R. Boynton, Samuel Patterson, and Ronald Hedlund that diffuse support for a state legislative institution showed a tendency to increase monotonically with education.[54]

The Supreme Court appears to be a somewhat distinctive institution. Among blacks, Supreme Court Legitimacy shows a clear tendency to increase with level of education, with a particularly sharp contrast between the college-educated and those whose formal education did not go beyond high school. The most striking finding is that for whites, Supreme Court Legitimacy shows a tendency to increase as level of education *decreases!*[55] And the

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cialization and the High School Civics Curriculum in the United States," *American Political Science Review*, 62 (September, 1968), 852–867.

[54] See page 170 in G. R. Boynton, Samuel C. Patterson, and Ronald D. Hedlund, "The Structure of Public Support for Legislative Institutions," *Midwest Journal of Political Science*, 12 (May, 1968), 163–180. Their measure of diffuse support is a scale built from the loadings of seven items on the first component of a principal components solution.

[55] This finding parallels that observed by Murphy and Tanenhaus for their 1966 national sample. Among subsamples of blacks and southern whites they found that, for the blacks, a unit change in Court Knowledge was associated with a +.45 change in Diffuse Support for the Court, whereas, for the whites, a unit change in the Court Knowledge was associated with a −.38 change in Diffuse Support for the Court. See Walter F. Murphy and Joseph Tanenhaus, *The Study of Public Law* (New York: Random House, 1972), p. 203.

sharp contrast is between those with only a grade-school education and those with more than a grade-school education. One plausible interpretation is that level of formal schooling increases the sensitivity of blacks and whites to the distinctively "liberal" orientation that has characterized the Court as an institution during the past two decades, with blacks favoring a "liberal" Court and whites opposed to a "liberal" court.

Personal legitimacy encompasses beliefs about the personal merit of political authorities. Generalized sense of trust-distrust in the political authorities refers to an interrelated set of beliefs about the way the authorities use the power that the state invests in them: Are they honest? Are they just? Are they benevolent? These are simple qualities for evaluating all human beings to which even young children—dimly aware of the power of the state—can relate. They remain important yardsticks by which adult citizens judge their political system.[56] Among adults, the power of the state is a very concrete fact of life—transmitted through the court system, the police, government regulations of all types. It will require little cognitive and moral sophistication to shift from evaluating political authorities on their own personal merit to evaluating them in terms of whether they use political power honestly, etc.

A pool of 24 items was developed to measure the degree to which government officials, the police, and the courts are perceived as using political power honestly, justly, and benevolently (see Appendix). Half of the items referred to national political authorities, the other half referred to local political authorities. It was hypothesized that these 24 items would all be related to a single dimension of trust-distrust

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Court Knowledge is strongly correlated with level of education. This contrast holds up for blacks as opposed to whites generally in their sample, although for whites as a whole the negative slope is not so steep (personal communication from Joseph Tanenhaus).

[56] The Political Trust scale developed at the Survey Research Center, University of Michigan, has been found to be homogeneous, among adults, in a number of studies. This scale measures the degree to which government officials in general are believed to wield power justly, honestly, and benevolently. Some of the items from it have been included in the Trust Political Authorities scale constructed here.

Table 8. Principal Components Solution: Trust in Political Authorities*

	Components**			
	I	II	III	IV
Nat'l gov't run for benefit all	.426	.398		
Nat'l. gov't officials are crooked	−.618		.424	
Can trust nat'l gov't to do right	.502	.428		
Nat'l gov't officials lie	−.538			
Police in U.S. deserve great respect	.588	−.331		
Police in U.S. would take bribes	−.633			
Police in U.S. treat people equally	.635		.420	
Police in U.S. unnecessarily violent	−.497	.408		
Courts in U.S. give fair trial	.619		.370	
Courts in U.S. are arm of police	−.493			−.334
Courts in U.S. sentence unequally	−.484			−.579
Local gov't wastes tax money	−.464		.380	
Local gov't run for benefit all	.454	.424		
Local gov't officials are crooked	−.665		.368	
an trust local gov't to do right	.640	.352		
Local gov't officials lie	−.601		.371	
ocal police deserve great respect	.605	−.368		
ocal police would take bribes	−.641	.342		
ocal police treat people equally	.633		.408	
ocal police unnecessarily violent	−.521	.430		
ocal courts give fair trial	.605		.306	
ocal courts are arm of police	−.556			
ocal courts sentence unequally	−.551			−.468
Percent of total variance =	32	09	07	05
Percent of explained variance =	61	15	14	10

* Loadings <.3 omitted.
** With eigenvalue >1.0.

in political authorities: when subjected to a principal component solution they all would load on the first component at $\gtrsim .500$. If children acquire diffuse affect for political authorities at an early age, and if adult socialization modifies but does not replace this early learning, then there might be a tendency, among adults, to generalize diffuse affect to both national and local levels of government.[57] The principal component solution for 23 of the items (the initial item being deleted), as shown in Table 8, confirms the above hypothesis.[58] Almost one-half of these items load at better than .600 on the first component; most of the remainder load at better than .500. Also, the first component accounts for slightly more than three-fifths of the explained variance.

The intercorrelation matrix for the measures of legitimacy is presented in Table 9. On the whole, they are moderately interrelated. Belief in the legitimacy of the Supreme Court is strongly correlated with belief in the legitimacy of Congress, but otherwise it is only slightly associated with the other legitimacy measures. The correlation pattern here is similar to that found for the student sample.[59] These data provide further support for the interpretation, advanced earlier, that the Supreme Court is perceived as a remote institution—less relevant than the other regime objects. The data also show that the dimensions of legitimacy, while sharing constraint, clearly are not all of a piece.

The diffuse support thesis suggests the following hypotheses:

PPV_3: Potential for Political Violence will be strongly and inversely correlated with Trust Political Authorities.

PPV_4: Potential for Political Violence will show a stronger negative correlation with Trust Political Authorities than with any other measure of diffuse support for the structure of political authority.

The correlation coefficients shown in the rightmost column of Table 9 confirm PPV_3 and PPV_4. The correlation of $-.464$ between Trust Political Authorities (TPA) and PPV is fairly strong in itself and is of considerably greater magnitude than the correlations between the other legitimacy variables and PPV.

A question of relevance for testing the model of potential for Political Violence is whether all the dimensions of legitimacy have independent effect on PPV or whether their association with PPV is indirect via one dimension of particular importance, namely, Trust in Political Authorities. Because there are so many missing data on two of the structural legitimacy items, investigation of the independent effect of structural legitimacy on PPV will be postponed for future analysis, in which certain subsamples will be isolated for investigation.[60] The regression of PPV against Trust Political Authorities and Legitimating Ideology, presented in Table 10, shows that the effect of Legitimating Ideology on PPV is entirely indirect, by virtue of association with Trust Political Authorities.[61] Inclusion of Legitimating Ideology in the PPV prediction equation adds nothing to the explained variance (r^2 for PPV regressed against TPA alone is .22): and the partial regression coefficient for Legitimating Ideology is less than two and one-half times its standard error, suggesting that it is probably due to chance. Thus, in the explanatory model, diffuse support will be measured by the Trust Political Authorities scale alone.

Belief that Political Violence has led to Goal Attainment. Apart from direct reinforcement, imitative learning (modeling) may produce belief that political violence has led to goal attainment in the past. I shall consider two sources of imitative learning: identification

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[57] M. Kent Jennings and Richard G. Niemi have reported data which indicate that adults (and high school seniors) have much greater trust in national government than in state and local government. This finding, however, is based on a fixed-choice question which forced respondents to *choose* the level in which they had the most "faith and confidence." If respondents are forced to rank the levels, such rankings could be somewhat arbitrary, reflecting, in part, the low salience of state and local government for most people. See M. Kent Jennings and Richard G. Niemi, "Patterns of Political Learning," *Harvard Educational Review*, 38 (Summer, 1968), 443–467.

[58] The initial item in the series defined a separate component all by itself when subjected to a principal components solution. This item read: "The people running the national government waste a great deal of the money we pay in taxes." There is little variance to it, as only 17 per cent of the sample expressed disagreement. That it is completely uncorrelated with the other 23 items in the series suggests that people may accept the idea that the national government wastes tax money as an unavoidable "fact of life."

[59] See Table 2 in Muller, "Correlates and Consequences . . . ," p. 402.

[60] For a discussion of the effect of missing data on regression analysis see A. J. Mackleprang, "Missing Data in Factor Analysis and Regression," *Midwest Journal of Political Science*, 14 (August, 1970), pp. 493–505.

[61] Among the students, Legitimating Ideology seems to be a more consequential legitimacy dimension. It is directly related to Avoid Protest Action, even when the other legitimacy measures are controlled. See Table 5 in Muller, "Correlates and Consequences . . . ," p. 408. The ethical principles on which the legitimating ideology is based are perhaps more meaningful to the students than to the average citizen.

Table 9. Legitimacy Intercorrelations (r) and Correlations (r) With Potential for Political Violence

	(PL)	(CL)	(SCL)	(TPA)	(PPV)
Legitimating Ideology (LI):	.217**	.211**	.020†	.413**	−.243**
Presidency Legitimacy (PL):		.417**	.257**	.248**	−.205**
Congress Legitimacy (CL):			.501**	.346**	−.253**
Supreme Court Legitimacy (SCL):				.233**	−.132*
Trust in Political Authorities (TPA):					−.464**

* $p < .01$.
** $p < .001$.
† $p > .05$.

with dissident groups in the society: and the demonstration effect of perceived successful or unsuccessful use of political violence for goal attainment by others in the society.

Dissident groups are defined here as groups whose explicitly stated goal is either to change or to separate from the existing regime, by means of violence if necessary. Dissident groups may be poorly organized, primitive mass communication systems may prevent their appeal from being widely disseminated, or they may be so discredited by past failure that men do not take them seriously: factors such as these will help to determine the appeal of dissident groups as a source of belief in the utility of political violence. If the group is well organized, if its appeal has been widely disseminated, and if it has not been discredited by past failure, it is likely that it will be perceived as an actual threat to the regime.

Another source of imitative learning will be individuals' evaluation of the effectiveness of past dissent behavior that has involved the use of violence. Agreement-disagreement with the ideas and actions of dissident groups in a society will presumably be associated strongly with belief in the utility-disutility of past violence in the society, because (1) support for the particular dissident groups will depend partly on perceptions of the group's past success with violent tactics, and such perceptions also will be a component of belief in the relative usefulness of violent tactics generally; and because (2) those who sympathize with dissident groups will absorb, to varying degrees, the antiregime ideologies of these groups, and adherence to such ideologies may lead individuals to interpret past antiregime violence as useful regardless of the "objective" outcome.

Table 10. Potential for Political Violence Regressed Against Legitimating Ideology and Trust in Political Authorities

Explained Variance (R^2) in PPV	Describing Variables	Partial Regression Coefficient*	F
.22	LI	−.026 (.018)	1.95†
	TPA	−.947 (.096)	97.58**

* Standard error in parenthesis.
** $p < .001$.
† $p > .05$.

Table 11 shows the distribution of this sample, by race, on items designed to measure degree of agreement-disagreement with the ideas and actions of dissident groups. The organizational weakness of Students for a Democratic Society, with its several major splits, is reflected in the low salience of the group: 51 per cent of the blacks have never heard of SDS; 37 per cent of the whites are unfamiliar with the group. It is curious that a majority of those blacks who say they are familiar with SDS agree with the ideas and actions of the group, since the majority of the blacks who are familiar with the Black Panther Party and the Black Muslims—groups that are considerably more relevant to the black experience than SDS—say that they *disagree* with these groups. Perhaps the proportion of blacks agreeing with SDS is inflated by individuals who are actually unfamiliar with SDS but are responding favorably to the connotations of the group's label. At any rate, the low visibility of SDS is the most significant aspect of the distribution of support for it here; because there are so many missing responses on the item, it has not been included in the Dissident Group Allegiance scale (See Appendix).

The American Communist Party is an example of a discredited dissident group. A minuscule 3 per cent of the blacks and 4 per cent of the whites agree with its ideas and actions. There is too little variance in this item to include it in the Dissident Group Allegiance scale.

The Black Panther Party and the Black Muslims exemplify highly organized and disciplined dissident groups which have managed to dis-

Table 11. Percentage Allegiance to Dissident Groups, by Race

Question: "There are many groups in America that try to get the government or the American people to see things more their way. I would like to get your feelings towards some of these groups. I am going to read off a list of groups and I would like you to tell me how you feel about the *ideas and actions* of each group.

Dissident Group		Blacks		Whites	
		Percent	Adjusted Percent*	Percent	Adjusted Percent*
Students for a Democratic Society	Disagree	18	36	43	68
	Neutral	07	14	07	11
	Agree	25	51	13	20
	DK	51		37	
	N=	(193)	(95)	(310)	(196)
American Communist Party	Disagree	70	93	82	93
	Neutral	03	04	02	03
	Agree	03	03	04	04
	DK	24		12	
	N=	(193)	(146)	(310)	(274)
Black Panther Party	Disagree	51	65	74	93
	Neutral	09	12	01	02
	Agree	18	23	05	06
	DK	22		20	
	N=	(193)	(151)	(310)	(248)
Black Muslims	Disagree	43	60	65	89
	Neutral	07	10	03	04
	Agree	21	29	05	07
	DK	30		27	
	N=	(193)	(136)	(310)	(225)

* Don't Know responses excluded.

seminate their appeal through various media. In this sample, almost as many say they are familiar with the Panthers as with the American Communist Party. Since Malcolm X's death, the Muslim leadership has tended to avoid the mass media, and consequently the group is less visible to this sample than the Panthers are; nevertheless, at least 70 per cent of both races are familiar enough with the Black Muslims to make an evaluation of the group. Support for these groups is very low among whites, but understandably somewhat higher among blacks. Although both groups have a considerable way to go toward mobilizing strong support in the black community, 18 per cent allegiance for the Panthers and 21 per cent allegiance for the Muslims represent a solid base from which to work. And this mass base is not totally black. Even though the Muslims are explicitly anti-white, and the Panthers are certainly not integrationists, both groups do appeal to 5 per cent of the whites. Undoubtedly this white support is a manifestation of allegiance to the antiregime emphasis in the ideologies of the Panthers and the Muslims.

Support for the Panthers correlates rather highly ($r = .703$) with support for the Muslims. The Dissident Group Allegiance scale is a summation of support for the Black Panther Party and the Black Muslims. The Panther and Muslim ideologies differ on certain basic points: the Muslims are fundamentally a religious sect, while the Panthers tend to regard themselves as a Marxist-Leninist political party.[62] Nevertheless, the ideologies of these groups do share a number of beliefs. Both groups ad-

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[62] A comprehensive analysis of the Muslim ideology appears in E. U. Essien-Udom, *Black Nationalism* (Chicago: University of Chicago Press, 1962); the Panther ideology has been articulated by their founder, Huey P. Newton, in "The Black Panthers," *Ebony*, 23 (August, 1969), 106–112.

Table 12. Percentage Belief in Efficacy of Past Violence, by Race

Question		Blacks Adjusted Percent*	Whites Adjusted Percent*
"Do you think the riots that have taken place in large cities around the country have helped or hurt the cause of Negro rights . . . "	Hurt	36	80
	Not Much Difference	22	08
	Helped	43	12
	N=	(173)	(303)
"Some white groups have fought with the police and destroyed public and private property in the cities in order to protest against American involvement in the war in Vietnam and other things that they dislike about American society. Do you think this has helped or hurt their cause . . . "	Hurt	45	75
	Not Much Difference	36	16
	Helped	19	09
	N=	(167)	(302)
"Some Negro groups have urged Negroes to arm themselves in order to be ready for shoot-outs with the police. Do you think this has helped or hurt their cause . . . "	Hurt	81	95
	Not Much Difference	15	04
	Helped	05	01
	N=	(170)	(299)

* Don't Know responses excluded.

vocate resistance to the present regime, resistance justified by ideologies which differ on certain basic principles and finer points, but which share a common interpretation of the regime as an unjust, physically oppressive, exploitative structure that must be disengaged from or overthrown. It is surely this common image of antiregime militancy, and not the more subtle ideological differences, that is salient for the public at large. (And even among those aware of the ideological differences, the groups may still be perceived as sharing enough in common to warrant a common feeling of allegiance.)

Table 12 shows the racial distribution on the items used to construct the Efficacy of Past Violence scale (see Appendix). The items cover the major kinds of violence that have occurred in America during the 1960s. Blacks are considerably more prone to interpret riots as efficacious than are whites; they are also moderately to slightly more prone to interpret the other forms of violence as having been useful. No more than 12 per cent of the whites believe that any of the political violence in the 1960s has been useful, but fully 42 per cent of the blacks feel that the riots have helped. Only 5 per cent of the blacks, however, believe that the Panther "self-defense" program has been useful. Both groups obviously perceive turmoil behavior as having shown more utilitarian payoff than interval war behavior.

The imitative learning version of the reinforcement thesis suggests the following hypotheses:

PPV_5: Potential for Political Violence will be strongly and directly correlated with Dissident Group Allegiance.

PPV_6: Potential for Political Violence will be strongly and directly correlated with Efficacy of Past Violence.

Table 13 shows that PPV_5 and PPV_6 are confirmed for this sample. Also, as expected, Dissident Group Allegiance (DGA) and Efficacy of Past Violence (EPV) are fairly highly correlated themselves ($r = .457$).

The DGA term will not be included in the test of the model of potential for political violence suggested by propositions 1–3. Dissident Group Allegiance not only is fairly highly correlated with Efficacy of Past Violence, it also correlates fairly highly ($r = -.467$) with Trust Political Authorities. Thus one might expect that Dissident Group Allegiance would provide relatively little information about PPV over and above Trust Political Authorities and Efficacy of Past Violence. In this sample, 15.9 per cent of the cases are "missing" on DGA, considerably more "missing data" than on any of the other variables in the model.[63] And the presence of a variable with a large amount of missing data in the multiple regression equation that will be used to estimate the parameters of the model, particularly if it is associated strongly with other describing variables in the

[63] Data are missing on the other variables as follows: SWG, 3 per cent; TPA, 1.6 per cent; EPV, 3.4 per cent; PPV, 0.8 per cent.

Table 13. Correlations (r) Between Dissident Group Allegiance, Efficacy of Past Violence, and Potential for Political Violence

	EPV	PPV
Dissident Group Allegiance (DGA)	.457*	.486*
Efficacy of Past Violence (EPV)		.458*

* $p < .001$.

equation, may produce distortion in the estimates of the parameters.[64]

Race and Age. In the United States, participants in the behaviors which define the potential for political violence continuum have *tended* to be young and black. I stress the word "tended" because it is obvious that many blacks and many of the young have not participated and some whites and some of the non-young have participated. One would expect to find some correlation between Potential for Political Violence and these variables, but not necessarily a strong correlation. (For correlation and regression analysis, Race can be treated as a "dummy" variable; dummy variables are assigned the values "0" and "1" to take account of the fact that each level may have separate effect on criterion variables.)[65] Table 1 showed that blacks were more likely than whites to rank medium and high on Approval of Political Violence and Intention to Engage in Political Violence. Table 14 shows that the correlation between Race and PPV ($r = -.364$) is of moderate magnitude and in the expected direction; and the correlation between Age and PPV ($r = -.267$) also is in the expected direction, although it is not very strong. The multiple correlation between both Race and Age and Potential for Political Violence is .439; Race and Age together account for about one-fifth of the variance in Potential for Political Violence.

One would not regard Race and Age, in and of themselves, as potentially having direct influence on Potential for Political Violence; rather, they are surrogate variables, standing for sets of unspecified factors that might have direct influence. If Race and Age are surrogates for any of the psychological variables that might directly influence Potential for Political Violence, one would expect that (1) Race and Age will show a nonzero bivariate correlation with the psychological variables, and (2) the partial correlation between Potential for Political Violence and Race or Age, controlling for the psychological variable, will be zero.[66]

Age is uncorrelated with Short-Term Welfare Gratification and Trust in Political Authorities (TPA) and shows only a weak negative correlation with Efficacy of Past Violence. Easton and Dennis have hypothesized that diffuse affect for the political authority structure will vary with age according to a cubic function rule, decreasing from childhood to the early twenties, then increasing steadily to the late fifties, and finally decreasing steadily after about age sixty, but never approximating the low of the early twenties.[67] The Age variable begins at 18 years. Thus, if the Easton-Dennis hypothesis holds, one would expect a fairly good linear fit between TPA and Age, but one would expect that a quadratic function rule would best describe the distribution of TPA by Age.[68] The zero linear correlation between TPA and Age leads us to suspect that these data do not confirm the Easton-Dennis hypothesis. A linear function rule accounts for only .004 of the variance in TPA; a quadratic function rule increases the explained variance to only .009. Figure 4 shows average TPA scores by seven age cohorts. These average scores suggest that only a quintic function rule would provide a decent description of the distribution of TPA by age in this sample. In line with the Easton-Dennis speculation, mean TPA does decrease from the youngest cohort to the next youngest cohort—although the amount of decrease is greater than one would expect on the basis of their model; and mean TPA also decreases from the next oldest to the oldest cohort. Mean TPA for the middle-age cohorts, however, looks very different from what one would expect on the basis of the decrease/steady-increase/decrease model: instead of an upward slope, there is a slight "V" pattern.

As one might expect, Race is quite strongly correlated with TPA ($r = .572$). Presumably, this relationship is due in part to differences in

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[64] See Mackleprang, pp. 499–503.

[65] A good discussion appears in Norman Draper and Harry Smith, *Applied Regression Analysis* (New York: Wiley, 1966), pp. 134–141.

[66] My definition of a "zero" correlation will be a value which is not statistically significant at the .01 level.

[67] See Easton and Dennis, *Children in the Political System* . . . , pp. 287–313.

[68] If the nadir of diffuse support for the political authorities occurred during respondents' early twenties, a quadratic function rule would provide the best fit, since any decrease from age 18 to the early twenties would be rather negligible; of course, if the nadir of diffuse support occurred during the late twenties and early thirties, then a cubic rule would provide an even better fit than a quadratic rule.

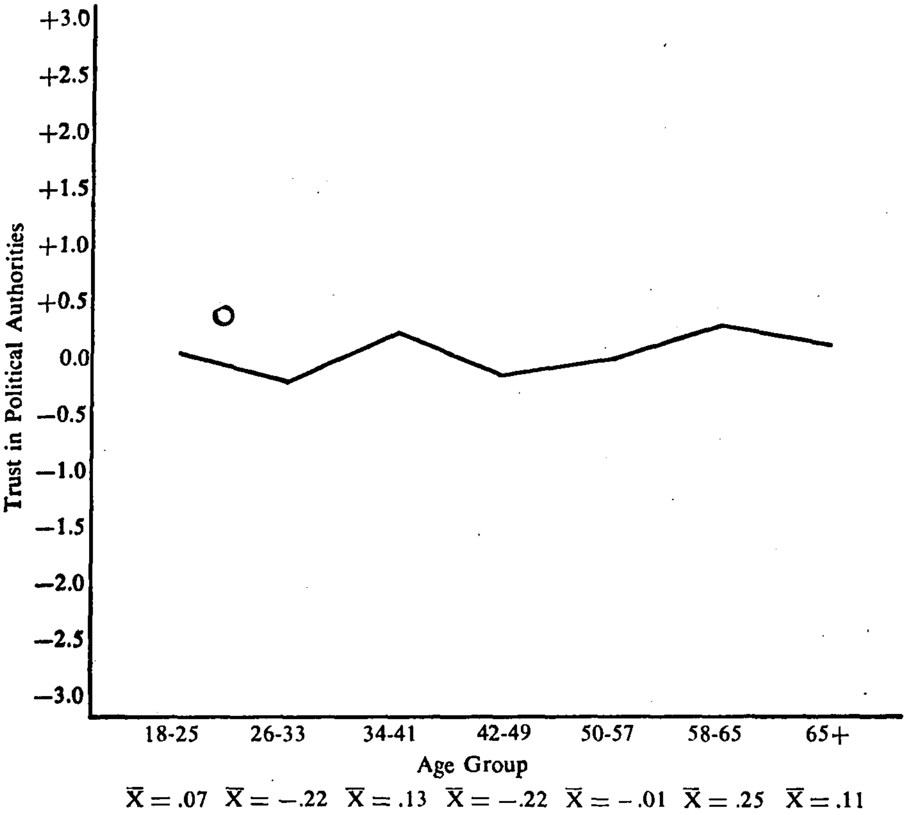

Figure 4. Mean Trust Political Authorities Score by Age Group

early political socialization.[69] It is surely due also to differing adult socialization experiences. Aberbach and Walker found that in Detroit blacks were more likely than whites to score low on a Political Trust index built from items similar to some of those included in the TPA scale.[70] They also found that among blacks, Personal Experiences of Police Mistreatment correlated (gamma) at −.43 with trust in national and local government officials, and a measure of the degree to which police mistreatment was believed to be a serious community problem correlated (gamma) at −.21 with trust in national and local government officials. The principal components solution presented in Table 8 showed that diffuse affect toward the police—as well as toward the courts—is correlated with diffuse affect toward government officials generally. Presumably, adult socialization experiences such as those reflected in the Aberbach-Walker measures of "personal experiences" and "recognition of serious community problems" affect generalized trust or distrust in the structure of political authority.

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[69] In two northern cities (Toledo, Ohio and Philadelphia, Pennsylvania) it has been shown that black children score lower than white children on various measures of diffuse support for the political authority structure. Findings from the Toledo study are reported on pages 293 to 301 of Schley R. Lyons, "The Political Socialization of Ghetto Children: Efficacy and Cynicism," *Journal of Politics,* 32 (May, 1970), 288–304; findings from the Philadelphia study are reported at pages 255–267 of Edward S. Greenberg, "Children and Government: A Comparison Across Racial Lines," *Midwest Journal of Political Science,* 14 (May, 1970), 249–275, and at pp. 338–340 of Edward S. Greenberg, "Black Children and the Political System," *Public Opinion Quarterly* (Fall, 1970), 333–345. In a southern urban area black children were found to have more negative attitudes toward the police than white children; however, although blacks did score somewhat lower on the SRC Political Trust scale than whites, the difference was not statistically significant. Also, a national sample, which contained a disproportionate number of black students from the South, found that black high school students were not less trusting (on the SRC Political Trust scale) of political authorities than white high school students. Findings from the southern urban area study are reported at pages 80 to 82 of Harrell R. Rodgers, Jr. and George Taylor, "The Policeman as an Agent of Regime Legitimation," *Midwest Journal of Political Science,* 15 (February, 1971), 72–86; findings from the national sample are reported at page 861 of Kenneth P. Langton and M. Kent Jennings, "Political Socialization. . . ." These studies suggest that black-white differentials in the acquisition of positive diffuse affect may be primarily a non-Southern phenomenon, at least in the recent past.

[70] See Table 2 in Aberbach and Walker, "Political Trust and Racial Ideology," p. 1204.

Race is less strongly correlated with Efficacy of Past Violence than with Trust in Political Authorities and least strongly correlated with Short-Term Welfare Gratification. When the variation in Potential for Political Violence which could be explained by the joint relationship between Race and each psychological variable is partialled out, the net relationship (first-order correlation) between Race and Potential for Political Violence (1) remains about the same as the gross relationship (zero-order correlation), controlling for Short-Term Welfare Gratification; (2) is substantially reduced from the gross relationship, controlling for Trust in Political Authorities; and (3) is moderately reduced from the gross relationship, controlling for Efficacy of Past Violence. In this sample, Race is clearly a surrogate particularly for Trust in Political Authorities: the partial r for Race and Potential for Political Violence, independent of Trust Political Authorities, is a very slight −.131, i.e., Race directly accounts for about 2 per cent of the variation in Potential for Political Violence.

Age is clearly not a surrogate for any of the psychological variables here. Since Age is uncorrelated with Short-Term Welfare Gratification and Trust in Political Authorities, controlling for them does not affect the zero-order correlation between Age and Potential for Political Violence: and a control for Efficacy of Past Violence produces only a slight reduction in the correlation between Age and Potential for Political Violence. These partial correlations suggest that in a model of Potential for Political Violence which includes TPA and EPV, Race may be an entirely redundant term: a surrogate for variables already in the model. But Age may well be a nonredundant term: a surrogate for unspecified variables not in the model.

Finally, the partial correlations between the psychological variables and Potential for Political Violence, controlling for Race and Age, show that each psychological variable is correlated with Potential for Political Violence independently of Race and Age, although the SWG-PPV partial correlation is very slight when Race is controlled for.

Models of Potential for Political Violence

On the basis of propositions 1–3 I shall propose the following hypotheses:

PPV_7: Potential for Political Violence will not be correlated with Short-Term Welfare Gratification independent of Trust in Political Authorities and Efficacy of Past Violence.

PPV_8: Potential for Political Violence will be inversely correlated with Trust in Political Authorities independent of Short-Term Welfare Gratification and Efficacy of Past Violence.

PPV_9: Potential for Political Violence will be directly correlated with Efficacy of Past Violence independent of Short-Term Welfare Gratification and Trust in Political Authorities.

A model of Potential for Political Violence corresponding to these hypotheses is depicted graphically in Figure 5. The single-headed arrows represent relationships that are thought to be direct and asymmetric, i.e., in which a change in the antecedent variable will influence or be attended by a later change in the consequent variable. The double-headed arrows represent relationships that could equally well be asymmetric or simply a result of the effect of common antecedent variables. Of course, for this sample, only guesses about covariation can be tested. Tests of guesses about the asymmetry of relationships would require at least a panel design.

Since Short-Term Welfare Gratification shows a nonzero bivariate correlation with Potential for Political Violence, it must also show a nonzero bivariate correlation with Trust in Political Authorities or Efficacy of Past Violence or both in order for PPV_7 to be confirmed. This condition is met: Short-Term Welfare Gratification correlates with Trust in Political Authorities at .332 and with Efficacy of Past Violence at −.219. In the Detroit study, a measure of relative gratification-deprivation similar to the SWG scale also correlated (gamma) positively with a measure of trust in political authority.[71]

The correlations shown in the right half of Figure 5 indicate that when TPA and EPV are controlled for, the correlation between SWG and PPV is approximately zero, thus confirming PPV_7. In fact, the partial correlation between SWG and PPV, controlling for TPA alone, is −.096, a value that is not statistically significant at the .01 level. For this sample, SWG correlates with PPV only because it also correlates with TPA and EPV—particularly TPA: it does seem that legitimacy sentiment

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[71] Gamma for blacks is .31; for whites it is .20. See Table 4 in Aberbach and Walker, p. 1209.

Table 14. Zero-Order and First-Order Correlations (r): Race, Age, and Variables in the Potential for Political Violence Model

	Zero-Order Correlation				
	Age	SWG	TPA	EPV	PPV
Race	.057†	.277***	.572***	−.416***	−.364***
Age		.104*	.064†	−.182***	−.267***

	Controlling For:		
First-Order Correlation	SWG	TPA	EPV
Race and PPV	−.326***	−.131**	−.231***

	Controlling For:		
First-Order Correlation	SWG	TPA	EPV
Age and PPV	−.267***	−.267***	−.217***

	Controlling For:	
First-Order Correlation	Race	Age
SWG and PPV	−.146***	−.214***
TPA and PPV	−.337***	−.465***
EPV and PPV	.358***	.432***

* $p < .05$.
** $p < .01$.
*** $p < .001$.
† $p > .05$.

operates to focus deprivation on, or deflect it away from, political targets.

TPA and EPV show a moderate degree of correlation in the expected negative direction. Perhaps Trust in Political Authorities operates to some extent as a "filter" through which men view violent dissent against the regime: if men feel that the regime is illegitimate, they may tend to interpret violent dissent against it as having been helpful to the cause of the dissidents, whatever the "objective" circumstances; if men feel that the regime is legitimate, they may tend to interpret violent dissent against it as having been detrimental to the cause of the dissidents. At any rate, PPV is inversely correlated with TPA, regardless of SWG and EPV, and PPV is directly correlated with EPV, regardless of SWG and TPA; thus, for this sample, PPV_8 and PPV_9 are confirmed.

The model depicted in Figure 5 can be expressed by the following equation:

$$V = \beta_0 + OS + \beta_1 T + \beta_2 E + \mu;$$

or:

$$V = \beta_0 + \beta_1 T + \beta_2 E + \mu.$$

Now, this is a straightforward linear additive model which assumes, of course, that there is no interaction between the Trust in Political Authorities (T) and Efficacy of Past Violence (E) terms. In other words, the model assumes that if, for example, T is held constant, the slope relating E to V (Potential for Political Violence) will be the same, regardless of the value of T.

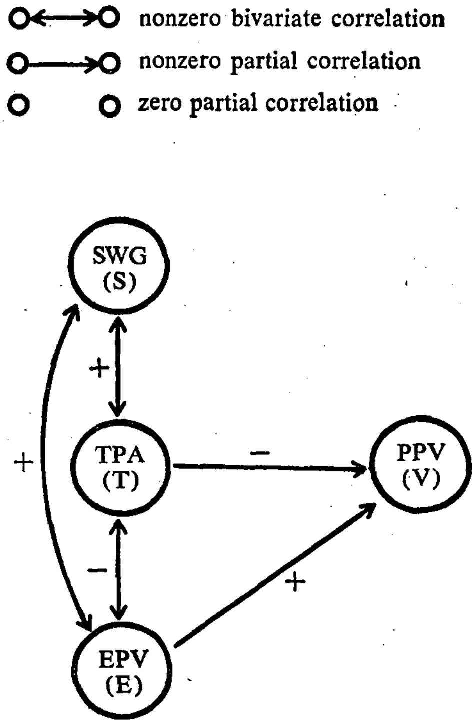
But what if T and E interacted in such a way that a multiplicative rather than an additive model would best describe their joint effect on V? If we reverse the direction of T so that the TPA scale ranges from "trust" at the low end to "distrust" at the high end, and label this reversed TPA scale "D" (Distrust Political Authorities), then a plausible PPV model might be:

$$V = \beta_0 + \beta_1 D + \beta_2 E + \beta_{12} DE + \mu.$$

This model assumes that if, for example, D is held constant, the slope relating E to V will have the same intercept, regardless of the value of D, but as D increases, the regression of V will be linear and will increase at a constant rate. Alternatively, we might allow for a nonlinear regression of V on E, as D increases, by postulating the model:

$$V = \propto D^{\beta} E^{\gamma} \mu.$$

Both these models are predicated on the notion that if either D or E are low, V will be low, and



Observed Correlations

$r_{ST} = .332^*$

$r_{SE} = -.219^*$

$r_{TE} = -.419^*$

$r_{SV \cdot TE} = -.071^\dagger$

$r_{TV \cdot SE} = -.318^*$

$r_{EV \cdot ST} = .305^*$

$^*p < .001$
$^\dagger p > .05$

Figure 5. A Simple Single-Stage Multivariate Model of Potential for Political Violence

that V will be high only if both D and E are high.

Table 15 shows four models of Potential for Political Violence, estimated by least squares regression. The variables used for estimation are:

V: PPV scale. Range: 0–10.
D: DPA scale (TPA scores reversed and an increment of "2" added to each score to eliminate negative signs). Range: .02–5.16.
E: EPV scale. Range: 0–6.
DE: DPA times EPV (increment of "1" added to each EPV score to eliminate zeros). Range: .02–35.87.
V_{ln}: Natural logarithms of PPV scores (increment of "1" added to each score to eliminate zeros). Range: 0–2.40.
D_{ln}: Natural logarithms of DPA scores (increment of "1" added to each score to eliminate fractions less than one). Range: .02–1.82.
E_{ln}: Natural logarithms of EPV scores (increment of "1" added to each score to eliminate zeros). Range: 0–1.95.
A: Age in years. Range: 18–90.

Model (1) is the linear additive model suggested by propositions 1–3. The fitted equation shows that both D and E are independent predictors of V: The partial regression coefficients for the "D" and "E" terms are well above their standard errors, and the partial F values are statistically significant; also, the accuracy of prediction with both describing variables in the equation is .30, almost fifty per cent greater than the accuracy of prediction for either variable alone. The tolerance value for E in the equation is high, indicating that E is a useful predictor of V, independent of D—in other words, D and E are not highly collinear. (The tolerance ranges from 0 to 1, where 0 would indicate perfect collinearity and 1 would indicate perfect linear independence.)

Models (2) and (3) postulate V as a multiplicative function of D and E. Unfortunately, it is difficult to arrive at reliable estimates of the parameters for model (2): inclusion of a cross-product term in the regression equation often will introduce substantial multicollinearity, and the multicollinearity effect, as noted by Robert P. Althauser, "will either depress or (less likely) inflate the regression estimates for the interaction or product term relative to those estimates for the individual terms."[72] For these

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[72] Robert P. Althauser, "Multicollinearity and Non-Additive Regression Models," in *Causal Models in the Social Sciences,* ed. H. M. Blalock (Chicago: Aldine-Atherton, 1971), p. 454.

Table 15. Regression Equations for Models of Political Violence

(Number of cases: 480) Model	R^2	terms	Partial Regression Coefficient*	β_0	Tolerance	F
(1) $V=\beta_0+\beta_1 D+\beta_2 E$	.30			.428		
		D	.736 (.091)			64.88 $p<.001$
		E	.462 (.063)		.82	54.45 $p<.001$
(2) $V=\beta_0+\beta_1 D+\beta_2 E+\beta_{12} DE$	.31			.788		
		DE	.142 (.050)			7.97 $p<.01$
		D	.407 (.148)		.48	7.61 $p<.01$
		E	.117 (.137)		.17	0.72 $p>.05$
(3) $V=\alpha D^{\beta} E^{\gamma}$						
(3′) $V_{\ln}=\alpha_{\ln}+\beta D_{\ln}+\gamma E_{\ln}$	.21			.368		
		$E_{\ln}$	.305 (.047)			42.51 $p<.001$
		$D_{\ln}$	.471 (.081)		.84	33.64 $p<.001$
(4) $V=\beta_0+\beta_1 D+\beta_2 E+\beta_3 A$	.34			1.600		
		D	.746 (.089)			70.50 $p<.001$
		E	.405 (.062)		.82	42.74 $p<.001$
		A	−.025 (.005)		.97	27.89 $p<.001$

* Standard error in parenthesis.

data, DE correlates with D at .725 and with E at .862; thus the tolerance value for inclusion of D in the equation is low (.48), and when E is considered, the tolerance is extremely low (.17). With all the terms in the equation, only the partial regression coefficients for DE and D are statistically significant. The presence of high multicollinearity, however, leads me to be skeptical of any substantive interpretation of the estimated parameters. At any rate, this particular nonadditive model is no more accurate in predicting V than the simple additive model.

Model (3) embodies the notion that V might be a nonlinear function of D for any given value of E and vice versa. Taking logarithms to the base *e* in model (3) converts the model into the linear form expressed in model (3′); the parameters of model (3′) can be estimated by least squares regression.[73] R^2 for model (3′) is only .21. Thus, model (1) affords fifty per cent better prediction of V than does model (3′). Therefore, model (3) may be rejected for these data. The linear additive model provides considerably better prediction than this multiplicative model.

Model (4) includes the age term (A) as an additional predictor of Potential for Political Violence (V). When A is entered into the equation, the partial slopes for D and E remain quite stable. As anticipated, Age is clearly a useful predictor of Potential for Political Violence in its own right. The high tolerance value for A shows that this term is not a surrogate for D and E. The partial slope for A is greater than two and one-half times its standard error, and the partial F-value is statistically significant, indicating that A does have independent effect on V. The R^2 of .34 for model (4) shows that the inclusion of A in the equation serves to increase the accuracy of prediction of V by four percentage points—not a striking increase, but not an increase to be ignored.

Race, for this set of variables, in large part "stands for" D and E. The tolerance value for Race in a regression equation that already includes D and E is .64, indicative of a fair degree of multicollinearity. Furthermore, the partial regression coefficient for Race in such an equation is not statistically significant.

The simple linear additive model with only two describing terms accounts for 30 per cent of the variance in PPV. To those accustomed to working with aggregate data, a model that accounts for 30 per cent of the variance in a response variable may not seem particularly impressive. But with a mass sample survey as the data base, an R^2 of .30 for a model containing only two describing variables is nothing to be ashamed of. On the other hand, it is clear that there is room for additional terms in the model.

In sum, the tests of these models suggest that relative deprivation—at least as measured here —has only a weak influence on individuals' propensity for violent dissent against the regime, and this influence is entirely dependent upon whether individuals also associate their feelings of relative gratification or deprivation (vis-à-vis welfare values) with their belief in the legitimacy or illegitimacy of the regime (as measured by trust in political authorities). Added together, belief in the legitimacy or illegitimacy of the regime, belief in the utility or disutility of violent dissent by dissidents in the past, and a person's age, have a fairly substantial influence on individuals' readiness for mobilization to engage in behavior that challenges the power of the state.

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[73] See, for example, Draper and Smith, *Applied Regression Analysis,* ch. 5.

Conclusion

A partial theory of why men engage in acts of unconventional dissent (up to and including revolution) against their governments has been advanced in this report. It was proposed that diffuse support for the structure of political authority, and belief that political violence has led to goal attainment in the past, would be directly related, and that relative deprivation would not be directly related, to potential for political violence. The data support these propositions. They suggest that the readiness to engage in revolution against the state will be greatest when a very low degree of trust in political authorities is combined with a very high degree of belief that the use of violence by dissident groups in the past has helped their cause; conversely, avoidance of dissent which even deviates from customary regime norms (such as legally permitted protest demonstrations) will be most likely when a very high degree of trust in political authorities is combined with a very low degree of belief that the use of violence by dissident groups in the past has helped their cause. Once these factors have been taken into account, relative deprivation does not seem to matter.

These data provide little support for an explanation of potential for political violence which ascribes strong—or any—direct effect to relative deprivation, or which casts relative deprivation as an important precondition that might be related to potential for political violence indirectly through effect on a factor such as belief in the legitimacy-illegitimacy of the regime. Relative deprivation (as measured by the Cantril Self-Anchoring scale) perceived with respect to welfare values—by far the most prevalent concerns among people all over the world —was found to be the *least* consequential predictor of potential for political violence; and a measure of long-term welfare gratification-deprivation showed no linear correlation with potential for political violence. Other studies also have found that relative deprivation, as measured by the Self-Anchoring scale, does not correlate very highly with measures of potential for political violence. We shall certainly want to experiment with other ways of operationally defining relative deprivation, and, as a future report will show, we shall want to pay close attention to the construction of dynamic measures of relative deprivation. We may also want, however, to revise some of our notions about the importance of relative deprivation as an antecedent of potential for political violence.

The linear additive model of potential for political violence, for which these data provide support, would predict an individual's *readiness* to engage in acts of unconventional dissent against the state. The precise occurrence of such behavior will, of course, depend on circumstances which serve as immediate cues for such behavior. An important class of such circumstances presumably would encompass the behavior of police officials, since belief in the degree to which police wield power justly, honestly, and benevolently is a component of the trust political authorities variable (the personal dimension of regime legitimacy). Among individuals with deep-rooted belief that the police wield power illegitimately, police behaviors that are perceived as congruent with such belief might be likely to precipitate political violence. Another class of precipitating circumstances includes legal proceedings, because diffuse affect toward the judiciary also is a component of trust political authorities.

In the 1960s the United States initiated a host of "Great Society" programs designed to improve the circumstances of its citizens with respect to welfare values. Blacks, the most disadvantaged group, have felt the impact of these efforts. On the average, they believe that their own personal welfare concerns (work situation, income, housing, children's education) have improved quite a bit since five years ago and will continue to improve substantially in the future. Yet there are still riots in America's urban centers, and the incidence of the internal war type of political violence has been steadily increasing. The partial theory of potential for political violence tested here suggests the following hypothesis: If the state is interested in reforms that will reduce the incidence of political violence, then welfare programs and so on are part of the solution (relative deprivation does show a moderate correlation with trust in the political authorities which, in turn, shows a strong correlation with potential for political violence), *but they are not the major part,* as commonly asserted. This theory suggests that a more effective policy response would be to introduce reforms designed to enhance trust in the justice, honesty, and benevolence of government officials generally, the police, and the judiciary. Attention certainly has been called to the need for reforms in these areas before, but very little has been done concretely. If this inaction continues, the incidence of dissent behaviors that challenge the power of the state should steadily increase in America.

Appendix. Construction of the Variables

Two kinds of multi-item indicators have been used in this analysis: (1) summation of scores derived from item loadings on a principal component solution; (2) summation of raw scores. Item nonresponse was handled as follows:

Short-Term Welfare Gratification (SWG):

Nonresponse to the present Work, Income, and Housing indicators was 9 per cent, 3 per cent and 3 per cent respectively. If a case was missing two or more scores it was excluded; otherwise the item mean was assigned. 488 respondents received an SWG score.

Long-Term Welfare Gratification (LWG):

Only 419 respondents received an LWG score. This was because the Work, Income, and Housing indicators were built from a combinatorial rule that included the past and future estimates, and there was more nonresponse on these estimates than on the present estimates. If a case was missing on two or more of the long-term Work, Income, and Housing indicators, it was excluded; otherwise the mean was assigned.

Trust in Political Authorities (TPA):

Nonresponse to the TPA item pool was minimal, reaching at most a level of 6 per cent. If a case was missing scores on more than half of the items it was excluded; otherwise the item mean was assigned. 495 respondents received a TPA score.

Efficacy of Past Violence (EPV):

Nonresponse to any of the EPV items was not greater than 7 per cent. If a case was missing two or more scores it was excluded; otherwise the item mean was assigned. 486 respondents received an EPV score.

Potential for Political Violence (PPV):

Nonresponse to the Approve Political Violence (APV) and Intention to Engage in Political Violence (IEPV) items pools was no greater than 1 per cent. If a case was missing four or more scores it was excluded from APV—the same rule applied to IEPV; otherwise the item mean was assigned. Cases missing on either APV or IEPV were excluded from the PPV scale. 499 respondents received a PPV score.

Potential for Political Violence

On the interview schedule, the format of the items included in the Approval of Political Violence and Intention to Engage in Political Violence scales was as follows: (1) Does R approve or disapprove of each dissent behavior? (2) How intense is R's approval-disapproval? (3) What are the reasons for R's approval-disapproval? (4) Would R himself engage in the dissent behavior? (5) If "yes" or "depends," under what circumstances would R engage in the dissent behavior? The series was prefaced with this brief paragraph:

> There are many possible ways for people to show their disapproval or disagreement with what the government is doing. I am going to describe a number of such ways. I would like to know which ones you approve of as ways of showing dissatisfaction with the government, and which ones you disapprove of.

The series consisted of these dissent behaviors:

(1a) How about taking part in protest meetings or marches that are permitted by the local authorities? APPROVE/DISAPPROVE/DEPENDS

(b) Under present circumstances would you take part in protest meetings or marches that are permitted by local authorities? YES/NO/DEPENDS

(2a) How about refusing to obey a law which one thinks is unjust, if the person feels so strongly about it that he is willing to go to jail rather than obey the law? APPROVE/DISAPPROVE/DEPENDS

(2b) Under present circumstances would you refuse to obey a law which you felt was unjust, and be willing to go to jail rather than obey the law? YES/NO/DEPENDS

(3a) How about trying to stop the government from functioning by engaging in sit-ins, mass demonstrations, take-overs of buildings, and things like that? APPROVE/DISAPPROVE/DEPENDS

(3b) Under present circumstances would you try to stop the government from functioning by joining sit-ins, mass demonstrations, take-overs of buildings, and things like that? YES/NO/DEPENDS

(4a) Now, suppose that all other methods have failed and the person decides to try and stop the government from functioning by engaging in violent protest demonstrations including actions such as fighting with the police and destroying public and private property?
APPROVE/DISAPPROVE/DEPENDS
(4b) Under present circumstances would you try to stop the government from functioning by joining violent protest demonstrations, including actions such as fighting with the police and destroying public and private property? YES/NO/DEPENDS

(5a) And how about trying to challenge the power of the government by arming oneself in preparation for battles with government authorities such as the police and the National Guard? APPROVE/DISAPPROVE/DEPENDS
(5b) Under present circumstances would you try to challenge the power of the government by arming yourself in preparation for battles with government authorities such as the police and the National Guard? YES/NO/DEPENDS

The preface and the first three approve-disapprove items were adapted from the Survey Research Center 1968 Election Study schedule.

For scalogram analysis, items 1a through 5a were dichotomized with "approve" and "depends" assigned a passing score and "disapprove" assigned a failing score; items 1b through 5b were similarly dichotomized with "yes" and "depends" treated as passing and "no" treated as failing. Since most of these questions pertain to illegal behavior, it was anticipated that many of those respondents who would actually approve of or engage in these behaviors might understandably be reluctant to divulge their true feelings to a strange interviewer. Thus, the "depends" category was included as a device to tap such potential approval and affirmative behavioral intention. On questions as sensitive as these, the assumption is that by assigning a passing score to the "depends" category, less distortion will result than if a "depends" category were excluded altogether or if it were assigned a failing score.

The scalogram analysis of these items is shown in Table A. The series was designed to fit patterns characterized by an ascending order of difficulty, ranging from least difficult at position 1a/1b to most difficult at position 5a/5b. The simple pass-fail responses to the Approval of Political Violence and Intention to Engage in Political Violence items fit the hypothesized difficulty patterns. The test of whether or not all five items in each group truly fit *cumulative* patterns of least to most difficult shows that the first two items in both scales contribute an inordinate amount of error.

Given the nature of the real world, the occurrence of "error" at these two positions is not surprising. Protest meetings that are permitted by the authorities sometimes produce the kind of behavior included at positions 3a/3b. Moreover, many citizens (particularly in a mass public sample) tend to perceive little difference between 3a/3b behavior and 1a/1b behavior regardless of the actual distinctions between protest activity which is legally justified and that which violates the law. Perusal of the open-ended responses to the 1a/1b series shows that a number of individuals explicitly link 1a/1b behavior with 3a/3b behavior.

Nevertheless, the observed response patterns across the full APV and IEPV item pools each show a coefficient of reproducibility (CR) which is acceptable, although in the case of IEPV the coefficient of scalability (the difference between CR and minimum marginal reproducibility divided by the difference between 1 and minimum marginal reproducibility) is rather low. Four-item response patterns (dropping the 1a/1b position) show higher reproducibility and scalability; but, of course, they reflect a lesser range of values along the dissent behavior continuum, and IEPV, in particular, becomes more markedly skewed than when built from the five-item pool. Furthermore, five-item response patterns show high reproducibility and scalability among the elite sample. Thus because the five-item scales do show acceptable reproducibility, provide greater discrimination on the dissent behavior continuum, and are directly comparable with the APV and IEPV scales for the elite sample, APV and IEPV for this sample have been constructed from the full five-item pools. Scale scores were assigned by summing the number of "passes."

Long-Term and Short-Term Welfare Gratification

Cantril's self-Anchoring Striving Scale was used to measure R's relative gratification-deprivation with respect to social welfare values. The Self-Anchoring Striving Scale is simply a picture of a ladder with each rung numbered; it

Table A. Scalogram Analysis of Approval of Political Violence and Intention to Engage in Political Violence

Approval of Political Violence Item Per Cent	1a	2a	3a	4a	5a
Pass	64	56	20	11	08
Fail	36	44	80	89	92

Errors	1a	2a	3a	4a	5a
	79	80	28	25	20

Total Errors = 232
Total Responses = 2495

Reproducibility = .91
Scalability = .61

Intention to Engage in Political Violence Item Per Cent	1b	2b	3b	4b	5b
Pass	36	34	10	05	03
Fail	64	66	90	95	97

Errors	1b	2b	3b	4b	5b
	79	71	19	11	06

Total Errors = 186
Total Responses = 2495

Reproducibility = .93
Scalability = .57

ranges from a low rung of 0 to a high rung of 10. As Cantril has described the administration of the scale, "a person is asked to define on the basis of *his own* assumptions, perceptions, goals, and values the two extremes or anchoring points of the spectrum on which some scale measurement is desired—for example, he may be asked to define the 'top' and 'bottom,' the 'good' and 'bad,' the 'best' and the 'worst.'[74] In this case R was asked to conceive the two extremes as "best" and "worst" with respect to four social welfare values as follows:

> Now I want to ask how you feel you and your family are personally doing. Here is the picture of the ladder. Suppose that rung number ten represents the best possible (ASK FOR EACH ITEM LISTED BELOW) for you and zero represents the worst possible (ASK FOR EACH ITEM LISTED BELOW) for you.
>
> With regard to your (ASK FOR EACH ITEM LISTED BELOW), where on the ladder do you feel you stand *at the present time?* Where would you say you stood *five years ago?* And where do you think you will be *five years from now?*
>
> a. work situation (you; the head of the household)
> b. total family income.
> c. housing accommodations
> d. chances of being able to get your children a good education

The short-term gratification-deprivation scores were obtained simply from R's present self-rating. The measure of gratification-deprivation with respect to children's education, item d above, was excluded from the two composite scales because of the large amount of nonresponse on this item since it only applies to people with school-age children. The work variable correlates with the income variable at .570 and with the housing variable at .463; the income variable correlates with the housing variable at .527.

The long-term gratification-deprivation scores were obtained according to the equation,

$$z = T_2 + (T_2 - T_1) + (T_3 - T_2);$$

which simplifies to,

$$z = T_2 + (T_3 - T_1);$$

where

z = the long-term gratification-deprivation score;
T_2 = the present gratification-deprivation score;
T_1 = the past gratification-deprivation score;
T_3 = the future gratification-deprivation score.

The equation adjusts each estimate of present discrepancy between achievement optimum and achievement for the direction in which R perceives discrepancy between achievement optimum and achievement to be moving over time. The scores range from a high of 20 (past = 0; present = 10; future = 10) to a low of −10 (past = 10; present = 0; future = 0). The high and low ends of this measure reflect temporal patterns in which discrepancy between achievement optimum and achievement is perceived to have expanded or contracted in a "great leap," followed by anticipation of future constancy. The work variable correlates with the income variable at .650 and with the housing variable at .474; the income variable correlates with the housing variable at .419.

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[74] Cantril, *The Pattern of Human Concerns*, p. 22.

Presidency, Congress, and Supreme Court Legitimacy

Presidency Legitimacy, Congress Legitimacy, and Supreme Court Legitimacy were measured with the ladder scale. In these instances R was asked to conceive the two extremes of the scale as "best" and "worst" in terms of the main purposes which he ascribed to each institution. The questions were phrased as follows:

> What do you think ought to be the main purposes of the (Presidency/Congress/Supreme Court) in the national government—that is, what kinds of things do you think the (Presidency/Congress/Supreme Court) ought to do? Anything else?

> Here is a picture of a ladder. Suppose that rung number ten represents the best possible fulfillment of these purposes by the (Presidency/Congress/Supreme Court) in the past and zero represents the worst possible fulfillment of these purposes by the (Presidency/Congress/Supreme Court) in the past. On the whole, as you think back over the years, how well do you think the (Presidency/Congress/Supreme Court) has fulfilled these purposes?

The scores on these scales are then the straightforward ladder ratings for each institution.

Legitimating Ideology

The questions which form the basis for this scale appear in Table 5 of the text. Each component item is a 7-point agree-disagree scale ranging from low to high as follows: "Disagree: Strongly," "Disagree: Moderately," "Disagree: Slightly," "Neutral: Indifferent," "Agree: Slight Slightly," "Agree: Moderately," "Agree: Strongly." The scores on the first, fourth, and sixth items were reversed so that all scales would share the same direction. The scores on the six items were then added together.

Trust in Political Authorities

Questions designed to measure R's generalized sense of trust-distrust in three categories of political authorities—government officials, the police, and judicial personnel—at both national and local levels of government were presented in fixed-choice agree-disagree format. The national government items were included in a portion of the interview schedule which dealt with attitudes toward various aspects of the national government. A 7-point agree-disagree scale served as a measuring instrument for each item; the national government items read as follows:

1. The people running the national government waste a great deal of the money we pay in taxes.
2. The national government is pretty much run for the benefit of all the people instead of for a few big interests looking out for themselves.
3. Most of the people running the national government are crooked.
4. The national government can be trusted to do what is right just about always.
5. Most of the people running the national government don't really mean what they say.
6. Considering everything, the police in the United States deserve great respect.
7. Most policemen in the United States would be willing to take a bribe.
8. On the whole, the police in the United States treat everyone equally, regardless of whether they are rich or poor, white or Negro.
9. Most policemen in the United States are unnecessarily violent when arresting suspected lawbreakers who represent what they personally dislike.
10. The courts in the United States give everyone a fair trial, regardless of whether they are rich or poor, white or Negro.
11. The courts in the United States act as an arm of the police instead of administering justice equally to all.
12. If the courts in the United States find someone guilty of committing a crime, the length of his sentence will depend on whether he is rich or poor, white or Negro.

At a later point in the interview schedule questions were asked about local government counterparts. The phrase "Waterloo city government" was inserted in place of "national government" on items 13–17 (counterparts of 1–5); the phrase "in Waterloo" was inserted in place of "in the United States" on items 18–21 (counterparts of 6–8); and "in Waterloo" was again inserted in place of "in the United States" on items 22–24 (counterparts of 10–12). Items 1–4 and 13–16 are analogues, varied slightly in order to fit the agree-disagree format used here, of items which have appeared on various University of Michigan Survey Research Center interview schedules in the past.

Construction of the Trust in Political Authorities scale has been discussed in the text. Scale scores were derived from the first component by the usual procedure, i.e., summing up the standardized item scores multiplied by the ratio of each item loading to the eigenvalue.

Dissident Group Allegiance and Efficacy of Past Violence

The Black Muslim and Black Panther Party items are straightforward 7-point agree-disagree scales measuring degree of agreement-disagreement with the ideas and actions of each group. Scores of Black Muslim Allegiance and Black Panther Party Allegiance were added together to form Dissident Group Allegiance. If nonresponse occurred on one of the items, the mean was assigned.

The full wording of the Efficacy of Past Violence items appears in Table 11 of the text. Each item was measured by a 3-point scale—"Helped," "Not much difference," "Hurt." The "riots" item correlates with the "fight police" item at .414 and with the "arm for shoot-outs" item at .338; the "fight police" item correlates with the "arm for shoot-outs" item at .331.

Hawks and Doves, Isolationism and Political Distrust: An Analysis of Public Opinion on Military Policy*

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Social scientists, like many others in academic life, have tended to be atypically "dovish" in their attitudes toward the Vietnamese War.[1] Partially as a consequence, they have been prone to assume that the major support for tough military policies comes from those segments of the public that are most nearly opposite to them in social characteristics: the less informed, the less educated, the working class, the poor—in short, those lower in socioeconomic status (SES). This assumption is reinforced by sporadic events such as the attack on peace marchers by construction workers in New York City—an event followed by a large countermarch emphasizing patriotism and support for the President. What could demonstrate more clearly the working-class support for tougher policies? If more scientific evidence is required, one can turn to the extensive empirical and theoretical work organized by Lipset under the heading of "working-class authoritarianism."[2] He argues persuasively that the socialization practices and social conditions that prevail in the working class make for less ego stability, shorter time perspective, and less conceptual sophistication—all of which combine to produce more hostile, intolerant attitudes as well as greater support for extremist and absolutist policies. Though Lipset does not address himself specifically to military policy, his analysis is certainly consistent with the assumption that persons of lower SES will support tougher policies.

With considerations such as these in mind, it comes as no small surprise to learn that recent public opinion surveys provide virtually no support for this assumption. Indeed, most findings flatly contradict it. For example, in June 1966, 41 per cent of those with a grade-school education favored immediate withdrawal from Vietnam as compared to only 27 per cent of those with a college education;[3] in February 1967, only 51 per cent of the grade-school educated favored continued bombing of North Vietnam as compared to fully 74 per cent of the college-educated;[4] in January 1968, only 44 per cent of the grade-school educated described themselves as "hawks," while 57 per cent of the college-educated did so;[5] and in September 1970, 61 per cent of the grade-school educated and only 47 per cent of the college-educated endorsed the Hatfield-McGovern amendment calling for an end to U.S. troop involvement in Vietnam by the end of 1971.[6] Based on a reanalysis of two polls conducted by the Survey Research Center during the Korean (1952) and Vietnamese Wars (1964), Hamilton concludes that: "Preferences for 'tough' policy alternatives are most frequent among the following groups: the highly educated, high status occupations, those with high incomes, younger persons, and those paying much attention to newspapers and magazines."[7] In a recent study of voting patterns on Vietnam referenda, Hahn, also, found that support for withdrawal was most prevalent in precincts characterized by lower SES.[8,9]

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[1] David J. Armor, Joseph B. Giaquinta, R. Gorden McIntosh & Diana E. H. Russell, "Professors' Attitudes Toward the Vietnam War," *Public Opinion Quarterly*, 31 (Summer, 1967), 159–175.

[2] See for example, Seymour M. Lipset, "Democracy and Working-class Authoritarianism," *American Sociological Review*, 24 (August, 1959), 482–501; Seymour M. Lipset, *Political Man* (Garden City: Doubleday, 1960), Chapter 4.

[3] *Gallup Opinion Index*, Report 13 (Princeton, New Jersey: Gallup International, Inc., June, 1966), p. 6.

[4] *Gallup Index*, Report 21, March, 1967, p. 5.

[5] *Gallup Index*, Report 32, February, 1968, p. 18.

[6] *The New York Times*, September 27, 1970, p. 18.

[7] Richard F. Hamilton, "A Research Note on the Mass Support for 'Tough' Military Initiatives," *American Sociological Review*, 33 (June, 1968), 439–445.

[8] Harlan Hahn, "Correlates of Public Sentiment about War: Local Referenda on the Vietnam Issue," *American Political Science Review*, 64 (December, 1970), 1186–1198.

[9] The belief that those lower in SES favor tougher policies cannot quite be dismissed as a complete myth. Some findings indicate that they are more prone to support extremely belligerent policies; e.g., the less educated were slightly more prone to endorse both the bombing of cities in North Vietnam (Gallup Index, Report 9, February, 1966), and the introduction of atomic weapons into the fighting (Gallup Index, Report 34, April, 1968). Such policies receive little support in any quarter (about 25 per cent nationally), and the slightly greater support for them among those

What is perhaps more surprising than the apparent positive association between SES and support for tougher policies is the fact that this association has been brought to our attention only recently in connection with the Vietnamese War. Does this mean it is a recent development—a peculiarity of the Vietnamese War not to be found in earlier periods of the Cold War? Hamilton's brief report on the Korean War suggests not, but the question is worth pursuing more thoroughly.[10]

The study to be reported in this paper, then, had a twofold purpose: first, to examine in detail whether the association between SES and support for tougher policies held for the period of the Korean War, and second, if such an association did hold, to explore some of the possible reasons for it.

Socioeconomic Status and Military Policy: Preliminary Findings

Surveys, Items, and Methods. Nine national sample surveys conducted by the National Opinion Research Center (NORC) between April, 1951 and October, 1952 were selected for use in this study.[11] These were obtained from the Roper Public Opinion Research Center. The particular surveys selected dealt almost exclusively with issues of foreign policy, and, in addition, contained a larger than usual number of items concerned specifically with military policy in Korea.

In order to test for associations with socioeconomic status, all military policy items were re-scored so that higher scores denoted support for their tougher alternative. The items, of course, differed in the toughness of their tougher alternatives (e.g., keep troops in Korea vs. bomb bases inside China). The items are listed below, roughly ordered from the softest to the toughest, and the precise scoring of each item is shown. (Numbers in parentheses refer to NORC numbers.)

1. (#302, 307, 314, 315, 320, 327) Do you think the United States was right or wrong in sending American troops to stop the Communist invasion of South Korea? (1) Wrong, (2) Don't know,[12] (3) Right.
2. (#302, 307) Do you think we should continue to keep our troops in Korea, or should we pull them out now? (1) Pull them out now, (2) Don't know, (3) Keep troops there.
3. (#315, 317, 320, 327, 332) If Communist armies attack any other countries in the world, do you think the United States should stay out of it, or should we help defend these countries like we did in Korea?[13] (1) Stay out, (2) Don't know, (3) Help defend them.
4. (#314, 315, 317) Which one of these three comes closest to your idea of what we should do in Korea: (1) Pull our troops out and bring them home, (2) Don't Don't know, (3) Continue the war on the present basis while the truce talks continue, (4) Attack the Communist forces with everything we have?
5. (#320, 327, 329, 332) Suppose the truce talks in Korea broke down completely. Which one of these three things do you *think we should do then?* (1) Pull our troops out and bring them home, (2) Don't know, (3) Keep our troops in Korea and hold the present line there, (4) Go ahead and attack the Chinese Communists.
6. (#302, 307, 320, 327, 329) Do you think U.S. airplanes should or should not cross the Korean border and bomb Communist supply bases inside China? (1) Should not, (2) Don't know, (3) Should.
7. (#302, 307, 320) Would you approve or disapprove of the United States giving the Chinese Nationalist government under Chiang Kai-Shek all the help it needs to attack the Communists on the mainland of China? (1) Disapprove, (2) Don't know, (3) Approve.
8. (#314, 317, 320, 327, 332) Do you think we will reach an agreement in the next month or so to stop the fighting in Korea? (1) Yes, (2) Don't know. If "No," do you know we should continue the truce talks or should we break them off now? (1) Continue, (2) Don't know, (3) Break off.[14]

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lower in SES probably reflects a lesser sensitivity to the massive civilian casualties associated with them.

[10] Hamilton, "Mass Support for 'Tough' Military Initiatives," pp. 439–445.

[11] These were: #302 (April, 1951), #307 (May, 1951), #314 (November, 1951), #315 (December, 1951), #317 (February, 1952), #320 (March, 1952), #327 (June, 1952), #329 (August, 1952), and #332 (October, 1952). The sample sizes ranged from 1200 to 1300.

[12] Here, as on all items, "don't know" was not explicitly offered as an alternative to the respondent, but a "don't know" response was recorded as such.

[13] On surveys #317 and 320 the question was worded slightly differently as follows: "If the Chinese Communists attack any other country in Asia, do you think the United States should stay out of it, or should we help defend that country? The effect was to increase very slightly (4 to 6 per cent) sentiment to stay out.

[14] Note that a respondent received a score of "1" either by expecting agreement within a month or by

9. Do you think the United States should go to war against Russia now? (1) No, (2) Don't know, (3) Yes.

Four demographic variables were used to measure socioeconomic status: Education, Occupational Status, Race, and Economic Level (the last represents an interviewer estimate of the respondent's standard of living). These four also were summed to form a single summary index of socioeconomic status (SES index). All variables were scored so that higher scores denoted higher SES.[15]

A measure of Level of Information could also be obtained on certain surveys through items that asked the respondent whether he had "read or heard about" specific events in the news (e.g., "have you read or heard anything about the recent troubles between the British and the Egyptians in Egypt?"). In general, no more than one such item appeared on any given survey and it was always scored so that higher scores denoted higher knowledge.

In addition to these variables, three other demographic variables were routinely tested for association with military policy preference: Sex [scored: (1) female, (2) male]; Age [scored (1) under 35, (2) 34 to 54, (3) over 54]; and Political Affiliation [scored (1) Republican, (2) Independent, (3) Democrat]. Preliminary tests indicated that Region, City Size, and Religious Affiliation had little or no relation to military policy preferences and, hence, their possible role was not pursued in the present study.

Historical Synopsis and Trends Over Time. Although the surveys analyzed spanned an 18-month period from April, 1951 to October, 1952, the military and diplomatic situation with respect to the Korean War remained reasonably stable throughout. General MacArthur was relieved of his command in April, 1951, shortly after the UN forces had fought their way back to the 38th parallel, after very nearly being pushed out of South Korea entirely by a sudden, massive influx of Chinese "volunteers." Subsequent congressional hearings firmly established that the UN objective was merely to defend South Korea and no longer to occupy and subdue North Korea. In June, preliminary feelers from the Soviet Union and China led to the opening of formal truce negotiations. The remainder of the period through October 1952 was characterized by a costly military stalemate along the 38th parallel while the truce talks dragged on interminably, bogged down over apparently trivial issues such as the conditions for a prisoner of war exchange. Thus, apart from the dismissal of McArthur and the opening of truce negotiations, both of which occurred early in the period, there were no noteworthy military or diplomatic watersheds.

Probably as a consequence of this, the marginal distributions of the military policy items remained remarkably stable throughout the period. The largest change was in the percentage of the public saying the U.S. had been wrong to send troops to Korea in the first place. This rose from a low of 27 per cent in April, 1951 to a high of 40 per cent in May, 1952, falling back slightly to 38 per cent in June. Other items showed even less fluctuation. The percentage saying that we should help defend another country attacked by Communist armies ranged between 61 per cent and 66 per cent throughout the period. Similarly, the percentage favoring bombardment of supply bases inside China ranged from 53 per cent to 61 per

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favoring continuation of the talks. In effect, it was assumed that respondents expecting agreement soon would want to continue the talks.

[15] Scoring was as follows: *Education*—(1) no formal schooling to 11th grade, (2) 12th grade to college; *Occupational Status*—(1) unskilled workers, (2) semiskilled workers, (3) skilled workers, (4) office workers, (5) business executives and managers, (6) professionals and semiprofessionals; *Economic Level*—(1) low or below average, (2) medium or average, (3) high or above average; *Race*—(1) nonwhite, (2) white. It might be noted that education was dichotomized only after analyses using more refined breakdowns revealed that no curvilinear relationships (or other interpretable departures from linearity) would be obscured as a consequence.

The above scoring was used when examining the separate effects of these variables. In the creation of the composite SES Index, all four variables were scored dichotomously: occupational status was dichotomized between blue and white collar, and economic level was dichotomized between low and medium. Scores on the summed index thus ranged from 4 to 8 and were recoded as follows for purpose of analysis: (1) low = scores of 4 or 5, (2) medium-low = score of 6, (3) medium-high = score of 7, (4) high = score of 8. The decision to treat race as an indicator of SES and, thus, to include it in the composite index was a departure from conventional practices about which some may have certain legitimate reservations. Although race clearly is associated with both economic and social status in this society, it is also associated with certain other important factors (such as cultural background and a history of discrimination) that have little to do with the conventional meaning of SES. While the inclusion of race might add a small amount of unwanted noise to our operational index of SES, nonetheless, the concept this index is intended to assess is the standard one, and throughout this paper the term "SES" is used in its conventional sense. To aid the reader who may wish to separate the effect of race from those of the more standard indicators, the data tables that follow will always report findings based on the SES index together with parallel findings based on its separate components.

cent. In general, the fluctuations between adjacent surveys were of the same magnitude as those across the entire period. In short, there were no discernible stable trends either in a softer or tougher direction, only a slight increase in the public's disenchantment with the original U.S. intervention in Korea.

Absence of a single Soft-Tough Dimension. Initial efforts to explore the demographic correlates of military policy preferences used the strategy of summing the separate military policy items appearing on each survey to form a single soft-to-tough scale. The scale was then correlated with the several demographic variables. This strategy proved highly unsuccessful in that it yielded quite different patterns of association across different surveys—evidently because of the somewhat different items that made up the scale on each survey. It is evident that such a strategy rests on the assumption that all military policy items fall on a single soft-to-tough dimension in such a way that agreement with certain policies logically implies disagreement with others (i.e., a respondent favoring soft policies—such as withdrawal from Korea—would be expected to oppose tough policies—such as bombing bases in China or declaring war on Russia). If such were the case, it would, indeed make little difference what items happened to be included on a particular survey and, hence, on a particular soft-tough scale. As it happens, however, this assumption proved totally false.

When the items on military policy were systematically intercorrelated, it became apparent that two distinct clusters were present: a Disengagement cluster containing all items whose softer alternative involved support of disengagement (e.g., pull out, stay out, wrong to send troops), and an Escalation cluster containing all items whose softer alternative involved a rejection of escalation (e.g., do not bomb China, continue the truce talks, disapprove aiding Formosa to attack China). Items in the first cluster had a median association of approximately .50, while those in the second cluster had a median association of approximately .40 (gamma coefficients). However, the median association between clusters was only about .10, indicating that escalation and disengagement are not nearly as opposite as one might assume—instead, they are essentially uncorrelated.[16] While certain segments of the public (evidently including professors) do tend to view withdrawal as the opposite of escalation, this is evidently not the case in general.[17] It might seem obvious that those who favored bombardment of bases inside China would be more inclined to believe that the United States had been right to send troops to Korea, but our results discredit the validity of such an apparently innocuous assumption—the median association between these two items across four surveys was only .09.

SES and Military Policy. With the distinction between Disengagement items and Escalation items firmly in mind, the pattern of associations between SES and military policy preferences shown in the first five columns of Table 1 becomes clear. Each entry in this table is a *median* gamma coefficient based on all the surveys in which a given item appeared. The first five items belong to the Disengagement cluster, while the last four belong to the Escalation cluster. It is readily apparent that SES relates differently to the items in each of these clusters: higher SES *is* associated with greater rejection of disengagement but *not* with greater support for escalation. This pattern is clearest for economic level, education, and information.[18] Race deviates slightly, showing a positive association with two items in the escalation cluster—whites being more proescalation than blacks.

The last two columns of Table 1 show the associations with sex and political affiliation. The pattern for sex resembles that for race with males being both more rejecting of disengagement and slightly more supportive of escalation. Surprisingly, political affiliation shows very little association with any of the military policy items, except that Democrats are, predictably, more likely than Republicans to call

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[16] The two items offering respondents three choices, ranging from "pull out" to escalate, had stronger associations with the items in the Disengagement cluster (medium gamma approximately .40), but were also associated with the items in the Escalation cluster (medium gamma approximately .20).

[17] For a careful analysis of the extent to which a sample of academicians do, in fact, order military policies on a soft-to-tough dimension, see Philip Brickman, Phillip Shaver, & Peter Archibald, "American Tactics and American Goals in Vietnam as Perceived by Social Scientists" in *Vietnam: Issues and Alternatives,* Walter Isard, ed. (Cambridge, Mass.: Schenkman Publishing Co., 1969), pp. 80–104.

[18] Item 4, which offered the respondent three alternatives, showed a slight curvilinear trend with education. Support for "pull out" decreased linearly, and support for "continue" increased linearly. Support for "attack," however, peaked at 12th grade and fell away slightly at the college level (5 to 9 percentage points depending on the survey). This trend was not present in the similarly worked item 5, nor in any of the items belonging to the escalation cluster.

Table 1. Demographic Correlates of Military Policy Items
(Median Gammas: NORC 302, 307, 314, 315, 317, 320, 327, 329, 332)

	SES Index	Econ. Level	Race (W+)	educat.	informat.	Sex (M+)	Polit. Affil. (D+)
1) Right to send troops to Korea	.14	.10	.20	.23	.18	.14	.18
2) Oppose pulling troops out of Korea	.26	.30	.38	.29	.50	.26	.05
3) Help defend another nation attacked by Comm. armies	.21	.17	.33	.26	.35	.27	.08
4) Policy for Korea: pull out/ continue/ attack	.17	.18	.46	.22	.21	.18	−.06
5) Policy if talks break down: pull out/ hold line/ attack	.25	.22	.37	.22	.27	.26	−.03
6) Bomb bases inside China	.05	.02	.41	.04	.08	.24	−.13
7) Aid Formosa to attack China	.04	.07	.36	−.05	.00	.13	−.13
8) Break off truce talks	−.11	−.07	.04	−.13	−.02	.10	−.07
9) Go to war with Russia	−.20	−.17	.07	−.30	−.20	.03	.09

the intervention in Korea by a Democratic administration "right." Beyond this, there is a *very* weak tendency for Democrats to be generally more supportive of administration policy in Korea—rejecting both disengagement and escalation more than Republicans. On the disengagement side, the weakness of this tendency is probably due to the fact that Democrats are of lower SES than Republicans, and hence somewhat more predisposed to favor disengagement. On the escalation side, the tendency to reject is stronger but hardly notable. In general, it would seem that opinion on military policy is not related to political affiliation in any obvious way.[19]

Discussion. During the Korean War, lower SES segments of the public *were* more "dovish" in the sense of being more prodisengagement. This finding accords with much available data on the Vietnamese War.[20] Our data caution us, however, against interpreting this "dovishness" as an opposition to escalation—Table 1 shows essentially no association between SES and support for escalation. Verba et al., working with data from the Vietnamese War, found a similar lack of association between SES and support for escalation.[21] Their findings do deviate from our own in that they obtained only a negligible inverse correlation between SES and support for "de-escalation," but this can probably be attributed to the fact that their "de-escalation scale" focused less on military disengagement and more on political compromise. (The Gallup data on "immediate withdrawal" and on "withdrawal by the end of 1971" cited earlier show the inverse association quite clearly.) In accord with the Korean data, they did find that race and sex were the strongest demographic correlates of anti-war sentiment—blacks and women being more opposed to escalation and more in favor of de-escalation.

Two possible explanations may be offered for the limited association between SES and toughness found in both these historical periods. According to the first, isolationism-internationalism is the critical intervening variable. People lower in SES can ordinarily be expected to have more limited horizons—to be more concerned with their immediate environment and less concerned with the rest of the world. Hence, they are more likely to manifest a parochial resistance to all forms of "altruistic" foreign involvement, especially those that require the sacrifice of American men and material half-way round the world for apparently vague internationalist values. While being more likely to support disengagement, people with such isolationist outlooks may not be more opposed to escalation, because escalation often has ambiguous implications for the level of sacrifice and involvement—it may appear to decrease casualties and hasten the end of the war, or it may appear to do just the opposite.

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[19] Two other demographic variables, occupational status and age, showed a pattern of association with military policy that resembled that of SES, but associations with the disengagement items were generally weaker and, in the case of age, disappeared entirely when education was controlled.

[20] See, for example, Hahn, "Correlates of Public Sentiment about War."

[21] Sydney Verba, Richard Brody, Edwin Parker, Norman H. Nie, Nelson W. Polsby, Paul Edman & Gordon Black, "Public Opinion and the War in Vietnam," *American Political Science Review*, 6 (June, 1967), 317–333.

The second explanation focuses on alienation from government as the critical intervening variable. Persons higher in SES can ordinarily be expected to be more attached to the mainstream of political life—to be more involved with government policy and to be more exposed to the mass media and other institutions that explain official policy and the rationales for it. As a consequence, persons of higher SES are more likely to support official government policy, not only because they better understand its purpose, but also because they are more likely to share with government officials the premises that make the policy seem reasonable[22] —for example, the value of internationalism. This explanation predicts that SES should be inversely associated with sentiment to disengage *and* with sentiment to escalate. The latter implication is not borne out by the data (except in the case of extreme forms of escalation such as going to war with Russia). Perhaps official government policy during a limited war includes a semitolerant attitude toward certain limited forms of escalation as a means of threatening the enemy and pressing for political settlement. At any rate, this second explanation does not appear to fit the data in Table 1 quite as easily as the first.

Both these explanations suggest particular intervening variables that ought to be associated both with SES and with military policy preferences. But the more general problem is that of finding the *correct* intervening variable—of discovering the broader dimensions that do, in fact, underlie support for disengagement or support for escalation. In order to pursue this problem, several of the surveys were subjected to a factor analysis. The intent of this analysis was threefold: to see whether items reflecting isolationism or alienation from government did, in fact, tend to load on factors that included escalation or disengagement items; to search for other dimensions that might be associated with military policy preferences; and to establish a frame of reference that would permit replication of further analyses across different surveys (that is, if similar factor structures emerged on different surveys, items with different wordings could then be equated by virtue of their high loadings on similar factors).

Dimensions Underlying Military Policy Preferences

Method. All fixed alternative items that dealt with domestic or foreign policy were included in the factor analysis. Demographic variables and items assessing level of information were excluded. The typical survey contained some 25 items of which more than 20 were included, almost all dealing with aspects of international relations: attitudes toward the UN, toward foreign aid, toward specific allies, and toward U.S. foreign policy both in general and in particular situations. Some items elicited estimates of the likelihood of events such as a major world war, or a new Communist invasion of another country. Others assessed the adequacy of governmental efforts to achieve certain objectives such as ending the Korean War or preventing a world war. A few dealt specifically with the issue of internal subversion: for example, disloyalty in the State Department, its extent, and the adequacy of efforts to eradicate it.

All military policy items were scored in the manner discussed earlier. All other items were scored so as to permit meaningful linear associations with each other. The alternatives associated with each item were assigned sequential integers begining with "1" in such a way as to capture their implicit ordering (e.g., "approve" to "disapprove," "too much" to "not enough," etc.). Almost all items offered the respondent either two or three fixed alternatives. Responses of "Don't know" were always assigned the value "2". Each survey was factor analyzed separately using the Pearsonian *r* correlation matrix and a principal components analysis (unities in the diagonal) that extracted factors by the principal axis method of Hotelling.[23] A ceiling of four was set on the number of factors to be extracted and the factor matrix was subjected to a varimax rotation.[24]

In all, eight surveys were analyzed in this way. Three were selected initially because of the wide range of items they contained. Five others were added because they contained items that seemed most relevant based on results from the first three. A large number of items appeared on more than one of these surveys, thus providing convenient points of reference for comparing the factors that emerged in each case.

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[22] For two more complete but somewhat divergent statements of this model, see Johan Galtung, "Foreign Policy Opinion as a Function of Social Position." *Journal of Peace Research*, 1 (1964: 3–4), 206–231; and William A. Gamson and Andre Modigliani, "Knowledge and Foreign Policy Opinions: Some Models for Consideration," *Public Opinion Quarterly*, 30 (Summer, 1966), 187–199.

[23] Arthur S. Couch & David J. Armor, The Datatext System: Preliminary Manual. (Harvard University, Department of Social Relations, multilithed, 1967), 265–304.

[24] Caspary factor analyzes some of the same surveys starting from a correlation matrix of gamma coefficients and obtains nearly identical results. See William R. Caspary, "Dimensions of Attitudes on International Conflict: Internationalism and Military Offensive Action," *Peace Research Society (International) Papers*, 13 (1970), 1–10.

International Interventionism and Administration Distrust. The factor analyses yielded results that were remarkably stable across all eight surveys. In each and every case, the two factors accounting for the largest amount of variance were clearly identifiable, very similar in content, and together accounted for approximately 30 per cent of the total variance. This is an extraordinarily large amount of common variance for a set of items not written with an eye to homogeneity. In addition, the disengagement items loaded heavily on the first of these factors, while the escalation items loaded somewhat on both, but more heavily on the second. The third largest factor to emerge was recognizably similar on three of the surveys (see footnote 34 below), but the fourth always differed. Here we shall be concerned only with the first two factors—those that emerged stably in all surveys.

Table 2 provides a capsule summary of the several factor analyses: it lists all items with a median loading (across the seven surveys) higher than .45 on Factor I or II, and also indicates the median loadings of all military policy items.[25]

Factor I has been labeled "International Interventionism" and appears to reflect the isolationist to internationalist dimension that was expected to emerge. The military disengagement items all load well on this factor, as do the first 22 items listed in Table 2. These items express the common theme that the United States should play a strong, active role on the world stage. Some advocate economic aid and technical assistance, others advocate military aid and deployment of troops in Europe, while still others advocate readiness to intervene in world trouble spots. Though these items have a strong internationalist flavor, it would be a mistake to view them as representing an idealistic sort of internationalism that desires involvement in the world in order to increase mutual understanding and cooperation. While some do have an idealistic quality, many are rather more antagonistic in tone and seem predicated largely on a desire to thwart world Communism. It is noteworthy, for example, that items expressing positive attitudes toward the UN did not load well on this factor.

The factor appears to reflect a uniquely American brand of internationalism—a brand that emerged following World War II when the United States abandoned entirely its earlier isolationist posture in order to assume the leadership of the "free world." In the postwar years, American foreign policy was a curious blend of altruism, activism, and self-rightousness—it had an almost crusading quality that caused its major thrust to be less a matter of promoting world cooperation, and more a matter of bringing American resources to bear on the evils of the world. During the early '50s, the major source of evil was believed to be Communism and the conditions that bred it. In effect, then, those who agreed with the items loading on Factor I tended to accept the major premise of U.S. foreign policy in this period: that it was the duty of the United States to intervene on the world stage in order to set things right.[26]

The second factor has been labeled "Administration Distrust" and bears only a vague resemblance to the "alienation from government" dimension that had been expected. Rather than reflecting a passive isolation from government policy and its rationales, the items that load high on this factor (items 23–39) appear to reflect a more active and bitter dissatisfaction with the performance of government on a wide variety of fronts. Their major thrust is that government officials were "not doing all they should" in coping with the problems that confronted them. This theme shades over into a belief that specific officials were incompetent and that certain persons in the State Department were disloyal and working against the interests of the country. Although most items deal with matters of foreign policy, the presence of one item referring specifically to domestic policy

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[25] The loading of a particular item on a particular factor remained relatively stable across different surveys. In almost all cases these loadings were within .10 of their median value. In no case were they more than .18 from their median value.

[26] Though somewhat damaged by recent events in Southeast Asia, this premise probably continues to dominate our foreign policy even today as it has for the past quarter of a century. (See William R. Caspary "The 'Mood Theory': A Study of Public Opinion on Foreign Policy," *American Political Science Review*, 64 [June, 1970], 536–547). The following excerpt from a recent speech about Vietnam by President Nixon illustrates well the special blend of altruism and self-righteousness:

> When men write the history of this nation they will record that no people in the annals of time made greater sacrifices in a more selfless cause than the American people scarificed for the right of 18 million people in a faraway land to avoid the imposition of Communist rule against their will and for the right of these people to determine their own future free of outside interference (Los Angeles Times, April 20, 1970).

Table 2. Summary of Factor Analysis of Eight NORC Surveys: Median Item Loadings on Two Largest Factors Under Verimax Rotation
(Based on NORC Surveys #302, 307, 314, 315, 317, 327, 329, 332)

Surveys on which items appear	Items	Factor I	Factor II
		International Interventionism	Administration Distrust
332	1. Good for U.S. to be active in world affairs	Median Loadings Larger than .45	*
332	2. Important for U.S. to cooperate closely with four allies		*
317, 329	3. Helping poor nations raise standard of living is good policy		*
314	4. Approve economic aid to W. Eur.		#
302	5. Approve of economic aid to friendly countries in Asia		*
315	6. Approve of giving further financial aid to England		*
315	7. Govt. should build defenses and aid allies despite higher taxes		*
315	8. U.S. should spend more to help W. Eur. build up defenses		#
302, 307, 314, 317, 329	9. Country has not made enough sacrifices for defense		*
317	10. Sending troops, military and economic aid to allies is helpful policy		*
314, 327	11. Approve of sending military supplies to W. Eur.		*
302, 327	12. Approve of sending military supplies to Asian allies		*
302	13. U.S. should spend more on military supplies for its allies		*
302, 307, 315	14. Approve of stationing U.S. troops in Eur. to build defenses		*
317	15. Stopping Comm. more important than staying out of war		*
329	16. Events in Middle-East are important to U.S.		*
327	17. U. S. should encourage rebellions in E. Eur.		**
317	18. Comm. seizure in Iran would be serious for U.S.		**
329	19. U.S. should do something if Comm. seize control in Iran		*
327	20. U.S. should help Comm. refugees from E. Eur.		*
332	21. U.S. defense program relies on raw material from other nations		*
315	22. U.S. can count on five allies for cooperation		*
all	23. Disapprove govt. handling of foreign affairs		*
314, 332	24. Disapprove of govt. handling of domestic policy	*	Median Loadings Larger than .45
317	25. Disapprove govt. handling of Comm. jailing of U.S. citizens	*	
314	26. Govt. has no definite plan for dealing with Russia	*	
317	27. Govt. did not do all it should to prevent fall of China	*	
315, 332	28. Govt. did not do all it should to prevent outbreak of Korean War	*	
332	29. Govt. not doing all it should to reach Korean accord	*	
317, 329	30. Govt. not doing all it should to prevent spread of Comm.	*	
315	31. Govt. not doing all it should to avoid another World War	*	
315, 329	32. State Dept. is not doing all it can to eliminate disloyalty	*	
329	33. Some officials in State Dept. are disloyal	*	
315	34. Disloyalty in State Dept. is hurting country	*	
317, 329	35. Acheson doing poor job and should be replaced	*	
302, 314, 315	36. Truman was wrong in dismissing MacArthur	*	
327, 329	37. Dissatisfied with allies cooperation with U.S. against Comm.	*	
329	38. Allies will not do their share in building up defenses	*	
314	39. W. Eur. not doing all it should to build up armed forces	*	
302, 307, 314, 315, 327	40. Right to send troops to Korea	.63	−.21
302, 307	41. Oppose pulling troops out of Korea	.73	−.02
327	42. Oppose pulling U.S. troops out of W. Berlin	.53	−.11
315, 317, 327, 332	43. Help defend another nation attacked by Communist armies	.63	.09
314, 315, 317	44. Policy for Korea: pull out/ continue/ attack	.45	.19
327, 329, 332	45. Policy if talks break down: pull out/ hold line/ attack	.62	.15
302, 307, 327, 329	46. Bomb supply bases inside China	.25	.61
302, 307	47. Give Formosa aid to attack China	.13	.69
314, 317, 327, 329	48. Break off truce talks now	−.15	.33
315, 327, 332	49. Go to war with Russia now	.06	−.03

** Median loading between .16 and .25.
* Median loading between −.15 and +.15.
# Median loading between −.25 and −.16.

(#24) suggests that the syndrome transcended the realm of foreign policy and reflected a distrust of the Truman administration more generally. As with Factor I, several items have a distinct anti-Communist flavor, but here the emphasis is either on the insufficiency of government efforts or on internal subversion. The themes expressed in these items are strongly reminiscent of the politics of Senator Joseph McCarthy, who was just emerging on the political scene in 1951.[27] Though not yet well enough known to have created this syndrome of distrust, it seems certain that he capitalized on its existence to vault himself into his later position of considerable power.[28]

Administration Distrust should be viewed as a special case of "political distrust" and should not be confused with a more generalized distrust of other people. Political distrust is a "basic evaluative orientation toward the . . . government,"[29] based on the belief that its outputs are unsatisfactory. As such, it is a *political* attitude conceptually distinct from a more general distrust of other people that borders on misanthropy or paranoia. The two may sometimes be associated empirically,[30] but more often they are not.[31,32] As Gamson notes, political distrust may be quite broad in its scope, encompassing the entire political community (as might be the case for a revolutionary movement), or it may be quite narrow, focusing only on the particular officials currently in power (as might be the case for a political party that is temporarily outside the government).[33] Administration Distrust is evidently very close to the latter end of this continuum. It reflects a distrust of the Truman administration and as will be shown shortly, it was most prevalent among Republicans.

The bottom of Table 2 shows the median loadings of the nine military policy items on the two factors. In general, those items associated with the Disengagement cluster (40 to 45) load heavily on International Interventionism, while those from the Escalation cluster (46–48) load somewhat on both but more heavily on Administration Distrust. Item 40, stating that the U.S. was right to send troops to Korea, has a notable negative loading on Administration Distrust, evidently because its wording suggested a fairly explicit endorsement of the administration. Item 49, advocating going to war with Russia, has no loading on either factor despite its moderate association with other items in the escalation cluster. Apparently, this extreme form of belligerence is beyond the scope of the two factors under discussion here.[34]

Discussion. The evident close connection between International Interventionism and opposition to military disengagement requires little discussion. It is apparent that opposition to withdrawal from Korea was only part of a broader posture based on the belief that the United States should play a strong, active role in the world stage. Those who favored such a posture supported the engagement in Korea, while those with a more isolationist stance tended to oppose it.

The connection between Administration Distrust and support for escalation is less easily understood. It is not surprising that Distrust should have been associated with a rejection of the administration's military policy, but it is less clear why this rejection should have taken the form of supporting escalation rather than disengagement. Possibly, the tendency to complain about insufficient government efforts, so evident in the Distrust items, led most easily into a support for escalation. Or possibly the anti-Communism component of the Distrust syndrome was more consistent with a support for escalation. While this latter explanation might seem the more plausible, it should be remembered that the Interventionist factor also had a component of anti-Communism without having any notable association with escalation. The anti-Communist component did take different forms in the two factors—a propensity to support active steps to combat world Communism, versus a propensity to criticize the government for being "soft on Communists"—but it is difficult to know what to make of these differences. At any rate, it is clear that people with more dissatisfied, distrusting attitudes toward the administration were more prone to favor escalation, while those with more trusting attitudes were prone to reject escalation in favor of less belligerent administration policies.

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[27] See Seymour M. Lipset, "Three Decades of the Radical Right," in *The Radical Right*, ed. Daniel Bell (New York: Doubleday, 1963), pp. 313–377; and Michael P. Rogin, *The Intellectuals and McCarthy* (Cambridge, Mass.: MIT Press, 1967).

[28] In April, 1953 almost six months after the last survey used in this study was conducted, fully 59 per cent of the public had no opinion on Senator McCarthy. Thus, it seems unlikely that he was responsible for creating the syndrome reflected by Factor II. (See Rogin, p. 232.)

[29] Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment," in *Ethics and Bigness: Scientific, Academic, Religious, Political, and Military*, ed. Harlan Cleveland and Harold D. Lasswell (New York: Harper, 1962), p. 64.

[30] Morris Rosenberg, "Misanthropy and Political Ideology," *American Sociological Review*, 2 (December, 1956), 690–695.

[31] See Edgar Litt, "Political Cynicism and Political Futility," *The Journal of Politics*, 23 (May, 1963), 320; and Joel D. Aberback & Jack L. Walker, "Political Trust and Racial Ideology," *American Political Science Review*, 64 (December, 1970), 1199–1219. (See especially pp. 1204–1205.)

[32] Survey #329 contained the following item, "Do you think most people can be trusted?" It had no association with Administration Distrust.

[33] William A. Gamsom, *Power and Discontent* (Homewood, Ill.: Dorsey, 1968), pp. 49–50. Gamson's discussion draws heavily on the work of Easton; David Easton, *A Systems Analysis of Political Life* (New York: Wiley, 1965).

[34] This item did have substantial loading on a third factor that emerged in three surveys. This factor reflected a general pessimism about the prospects of avoiding war and attaining a stable peace: e.g., another world war can be expected within 5 years, a new communist attack on another nation is likely soon, an agreement in Korea is unlikely to be reached soon, a long period of peace cannot be expected to follow an armistice in Korea, etc. This suggests that the desire to go to war with Russia reflected a sentiment to "get it over with," since matters were likely to continue to deteriorate leading ultimately to world war anyway. See also Caspary, "Dimensions of Attitudes on International Conflict," for other speculations on this item.

Four Types of Individuals and Their Military Policy Preferences. The independence of the two factors that emerged is quite consistent with the earlier noted lack of association between sentiment to escalate and sentiment to disengage. Taken together, these results suggest a useful typology for organizing public opinion on military policy. The Korean War public can be dichotomized in two ways on the basis of two attributes: i.e., Isolationists vs. Interventionists and Trusters vs. Distrusters. Since each attribute is independent of the other, the two can be used simultaneously to delineate four basic types of people: Trusting-isolationists, Distrusting-isolationists, Trusting-interventionists, and Distrusting-interventionists. In reality, of course, the public cannot be subdivided so clearly—many persons exemplified one of these types only marginally. Nonetheless, the factor analysis suggests that an ideal representative of each type should have had a characteristic preference ordering among alternative military policies. To keep matters simple, we shall focus on three alternatives in particular: disengagement, current policy, and escalation.

Table 3 summarizes the expected preference ordering of each of the four types. This table is based on the following simple assumptions derived from the factor analysis: Interventionists should oppose disengagement while Isolationists should favor it; Distrusters should favor escalation and tend to reject current policy, while Trusters should oppose escalation and tend to accept current policy. As can be seen from Table 3, the preference orderings that emerge are not only different for each type, but also not necessarily linear along a soft-to-tough dimension—e.g., Distrusting-isolationists should prefer both disengagement and escalation to current policy.

Methods. In order to test these expectations systematically, it was necessary first to select respondents who could represent each of the four types and then to examine their responses to the several military policy items. To accomplish this, all military policy items were first set aside to be used as dependent variables. The remaining items from each survey with loadings greater than .45 on Interventionism or Distrust, respectively, were separately summed to create Interventionism and Distrust scales for each survey. In each case, respondents' scores on these scales were used to subdivide the sample into terciles. Those scoring in the middle tercile were then discarded so as to retain only the respondents who scored either high or low on both Interventionism and Distrust. These remaining respondents were then sorted into the four desired types. The intent in dropping the middle terciles was, of course, to eliminate respondents who fitted the typology only marginally. In practice, this led to concentrating on 45 to 50 per cent of the sample depending on the particular survey.[35]

Table 3. Expected Preference Orderings of Four Types on Military Policy

	Isolationists		Interventionists	
	Trusting	Distrusting	Trusting	Distrusting
Favors	disengagement	disengage./escalat.		escalation
Accepts	current policy		current policy	
Rejects		current policy		current policy
Opposes	escalation		disengage./escalat.	disengagement

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[35] Since this analysis is based on a selected portion of the sample, it should be clear that the results cannot be generalized to the entire population. It was conducted to see whether those segments of the public who best represented the four types did, in fact, have military preferences of the sort expected.

Table 4. Interventionism, Distrust, and Military Policy[a]
(In Per Cent)[b]

		Isolationist		Interventionist		Full Sample
		Trust	Distrust	Trust	Distrust	Marginals[c]
(NORC 302, 4/51)	n=	(183)	(170)	(206)	(170)	Phi=.39
1) Pull troops out of Korea or keep there?	Pull out	28	38	9	11	21
	Don't know	20	9	3	9	11
	Keep there	52	52	88	81	70
(NORC 317, 2/52)	n=	(166)	(166)	(196)	(205)	Phi=.50
2) Help defend other nations attacked by Comm. armies?	Stay out	61	65	20	15	38
	Don't know	10	5	5	3	7
	Help defend	28	30	75	82	56
(NORC 317, 2/52)	n=	(166)	(166)	(196)	(205)	Phi=.51
3) What should we do now in Korea?	Pull out	52	43	15	8	28
	Don't know	10	4	4	5	6
	Cont. as now	21	10	48	28	27
	Attack Ch. Cm.	17	43	33	60	39
(NORC 332, 10/52)	n=	(174)	(141)	(169)	(180)	Phi=.53
4) If the talks break down, what should we do then?	Pull out	41	54	10	21	30
	Don't know	18	9	6	7	11
	Hold pres. line	27	11	60	24	30
	Attk. w/evry th.	14	26	24	48	29
(NORC 315, 12/51)	n=	(71)	(191)	(166)	(99)	Phi=.60
5) Was U.S. right or wrong to send troops to Korea?	Wrong	39	71	7	27	34
	Don't know	16	6	2	7	9
	Right	45	23	91	67	57
(NORC 302, 4/51)	n=	(183)	(170)	(205)	(169)	Phi=.40
6) Should U.S. bomb supply bases inside China?	Should not	45	27	35	12	27
	Don't know	27	9	15	4	18
	Should	28	64	50	84	56
(NORC 302, 4/51)	n=	(184)	(169)	(206)	(170)	Phi=.44
7) Approve U.S. aid Formosa for attack on China?	Disapprove	46	28	34	11	25
	Don't know	30	13	9	5	17
	Approve	24	59	57	85	59
(NORC 317, 2/52)	n=	(166)	(165)	(194)	(205)	Phi=.33
8) Should we break off the truce talks now?	Continue	48	38	72	53	55
	Don't know	26	12	8	6	13
	Break off	27	50	20	41	32

[a] Upper and lower terciles of Interventionism and Distrust Scales are used to create four types—middle terciles are dropped.
[b] Columns in each table sum to 100%.
[c] Phi coefficients are based on the figures and sample sizes shown in the body of the tables.

Policy Preferences of the Four Types. Table 4 shows the responses of the four types to eight separate military policy items.[36] In examining the response to each item, one must be careful to identify the implicit alternatives being offered to a respondent, because these are critical to understanding the pattern of responses.[37] The data in Table 4 are generally quite consistent with the expectations summarized in Table 3. Items 3 and 4, which offer a respondent the full range of alternatives, match Table 3 almost perfectly. The only items that deviate notably from the expected pattern are 6 and 7. In their responses to these items, the two distrusting groups were, as expected, the most prone to favor bombing China and aiding Formosa to attack China. It is also apparent, however, that these escalatory policies were more appealing to Interventionists than Isolationists—the former are seen to be notably more supportive of them across comparable levels of distrust. Evidently, these escalatory policies were viewed by some as being merely a more aggressive form of interventionism.

The results of this analysis confirm that the Trusting-isolationists were the most "dovish" sector of the Korean War public while the Distrusting-interventionists were the most "hawkish"; the preference orderings of both were linear on a soft-tough dimension, though opposite in direction. Thus, more than 50 per cent of the first group favored immediate withdrawal and less than 30 per cent favored bombing China, while only 11 per cent of the latter group favored withdrawal and fully 84 per cent favored bombing China. Presumably, these two groups contribute most to the general association between SES and toughness reported by Hamilton.[38] (As we shall show, these two groups are the most widely separated on measures of SES.)

The military preferences of the remaining two groups cannot be ordered on a simple soft-to-tough dimension. The Distrusting-isolationists were evidently oriented toward "getting it over with." They were equally inclined to support disengagement or escalation while being distinctly opposed to administration policy. Thus, nearly 40 per cent of this group favored immediate withdrawal from Korea, 64 per cent favored bombing China, and fully 71 per cent believed the administration's decision to intervene in Korea was wrong. On the other hand, the Trusting-interventionists were primarily oriented toward "carrying on." They opposed both disengagement and, to a lesser extent, escalation, while being most in favor of current policy. Thus, nearly 90 per cent of this group opposed immediate withdrawal, 50 per cent failed to support the bombing of China, and fully 91 per cent endorsed the original U.S. intervention in Korea.

If these two groups (or ones comparable to them) can be identified in other wartime contexts, they may help to explain one of the most puzzling features of public opinion on military policy: namely, that a wartime administration can usually manage to increase its support *either* by stepping up *or* by stepping down the level of military activity.[39] (For example, public support for President Johnson's handling of the war tended to increase following each cessation and resumption of the bombing of North Vietnam. Similarly, President Nixon's support increased both in response to his announced plan for "phased withdrawal" and following the Cambodian invasion.) The Trusting-interventionists presumably contribute to such a phenomenon by being willing to follow the administration wherever it leads, while the Distrusting-isolationists contribute by being inclined to support any move toward either escalation or disengagment. The upsurge in support is, of course, short lived because the Distrusting-isolationists should tend to revert to their disgruntled anti-administration position when the new initiative fails to "get the war over with."[40]

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[36] Although this analysis was carried out on all surveys that had been factor analyzed, some contained too few Interventionism or Distrust items to create entirely reliable scales. The results to be reported here come from surveys #315, 317, and 332 all of which contained at least four items on each scale. Survey #302 is also used, even though its Distrust scale only had two items, because it contained some military policy items not found on the other three surveys. In general, the results from these surveys were similar to those that emerged on the other surveys containing less adequate scales.

[37] Items 1 and 2 offer the respondent a choice between disengagement and some version of current policy. Items 3 and 4 offer the full range of alternatives. Items 5 offers a choice between disengagement (phrased as an explicit rejection of current policy) or an explicit endorsement of current policy. Items 6–8 offer a choice between escalation and some version of current policy. In item 8, however, the escalation alternative is more ambiguous. Presumably, breaking off the truce would necessitate simply fighting it out in some manner. Still, this policy has certain overtones of disengagement which might explain its apparent appeal to the Isolationist groups.

[38] Hamilton, "Mass Support for Tough Military Initiatives."

[39] For discussion of this feature, see Philip Converse and Howard Schuman, " 'Silent Majorities' and the Vietnam War," *Scientific American*, 222 (June, 1970), 21–22; and Milton J. Rosenberg, Sydney Verba and Philip E. Converse, *Vietnam and the Silent Majority: The Dove's Guide* (New York: Harper & Row, 1970), pp. 26–30.

[40] During the 1968 campaign, Distrusting-isolationists

Another notable feature of public opinion on military policy is the tendency for opposition to wars to increase most strongly following military setbacks. (For example, the proportion of the public believing that the U.S. intervention in Korea had been a mistake rose from 20 per cent to 49 per cent following the disastrous consequences of China's entry into the war.[41] The Tet offensive of 1968 had a similar though less dramatic impact.) Presumably, it is the two isolationist groups that contribute most to this effect. They are the most likely to become "war-weary" because they are oriented primarily to the costs of foreign involvements, whereas the interventionists are oriented more to achieving certain political objectives and hence are more likely to hold on until these objectives are achieved.

Both isolationism and distrust of the administration might be expected to increase over time as a function of the length and frustrating nature of a foreign war. A careful study by Caspary, however, shows that isolationist sentiments have displayed little tendency to fluctuate as a function of specific events over the two decades following World War II.[42] Our own data support his findings—during the 18 months covered by this study, there was essentially no change in the marginal distributions of items loading on the Interventionist Factor. Hence, it seems more accurate to say that while the level of isolationist sentiment does not change much over time, those who harbor such sentiments become increasingly likely to favor disengagement as the war becomes more frustrating. On the other hand, administration distrust does increase over the course of a frustrating war. For example, approval of President Truman's "handling of his job as president" fell from 46 per cent at the outbreak of the Korean War to a low of 23 per cent in the midst of the military and diplomatic stalemate. Similarly, President Johnson's popularity slipped from 71 per cent at the start of his second term to a low of 36 per cent just prior to his decision not to seek reelection.[43]

Interventionism, Distrust, and Socioeconomic Status

We now come full circle back to SES in order to examine its association with Interventionism and Distrust.[44] The first two columns of Table 5 show these associations in the form of median gamma coefficients based on all available surveys. As expected, Interventionism is positively associated with all measures of SES. Information is the strongest correlate of interventionism; sex has some association (with males being more pro-intervention); and political affiliation has virtually no association. Though the correlations with SES are not as strong as might have been hoped, it is clear that Interventionists tended to be of higher SES than Isolationists. This is consistent with both the models set forth earlier. In effect, Interventionists combined a traditional internationalist outlook with an acceptance of the basic premises of American foreign policy. Hence, persons higher in SES would be expected to be more interventionist both because their outlook on the world is likely to be less parochial, and because they are likely to be more in touch with the mainstream of political life.

The socioeconomic correlates of Administration Distrust, shown in the second column of Table 5, are considerably more surprising. Distrust turns out to be *positively* associated with both SES and information in much the same manner as Interventionism. The strongest correlate of Distrust, however, is political affiliation—Republicans were decidedly more distrusting than Democrats. It is plausible to argue that the positive associations between SES and Distrust arose because Republicans were both more distrusting and higher in SES than Democrats. Controlling for political affiliation should then cause the associations to disappear. The last three columns of Table 5 show that this does not occur.[45] While the associations are greatly attenuated among Democrats and Independents, they become even more pronounced

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might well have accounted for the surprising number of voters who supported McCarthy or Kennedy in the primaries and then shifted to Wallace in November (Philip E. Converse, Warren E. Miller, J. G. Rusk and A. Wolfe "Continuity and Change in American Politics: Parties and Issues in the 1968 Election." *American Political Science Review*, 63 (December, 1969) pp. 1083–1105). All three candidates would have appealed to voters who distrusted President Johnson and wanted to end the war, though Wallace best embodied their military policy preferences with his "get out or do it right" platform. Consistent with this view, Rosenberg et al., report that, "Those who voted for McCarthy in the New Hampshire primary were more dissatisfied with Johnson's policies on Vietnam than those who voted for Johnson. But among McCarthy voters, those who were dissatisfied with Johnson for not pursuing a *harder* line outnumbered those who wanted a withdrawal by a margin of nearly three to two." (Rosenberg et al., p. 49.)

[41] Hazel Erkine, "The Polls: Is War a Mistake?" *Public Opinion Quarterly*, 34 (Spring, 1970), p. 138.

[42] Caspary, "Mood Theory."

[43] *Gallup Index*, Report 56, February, 1970, pp. 8–16.

[44] The Distrust and Interventionism scales described in the preceding Methods Section were used in this analysis. Both were trichotomized at the terciles. The two scales proved to be as uncorrelated as the Factors on which they were based—their median intercorrelation across the eight surveys used was only −.07.

[45] Only those demographic variables with notable

Table 5. Demographic Correlates of Interventionism and Distrust
(Median Gammas: NORC 302, 314, 315, 317, 320, 327, 329, 332)

	Full Samples		Only Republicans	Only Democrats	Only Independents
	International Interventionism	Adminis. Distrust	Adminis. Distrust	Adminis. Distrust	Adminis. Distrust
SES Index	.24	.20	.27	.08	.04
Education	.28	.24	.27	.11	.09
Occup. Status	.19	.14			
Econ. Level	.24	.25	.30	.08	.08
Race (White+)	.33	.34			
Sex (Male+)	.18	.07			
Age	−.18	.08			
Polit. Affil. (Dem+)	.07	−.44			
Information	.38	.29	.36	.11	.12

among Republicans. Clearly, higher-status, better-informed Republicans were the most distrusting sector of the public during this period of the Korean War.

SES, Distrust, and Political Affiliation. A number of plausible explanations can be offered for these findings. One possibility is that higher-status Republicans were simply more "Republican" (conservative) and, hence, more prone to distrust a liberal Democrat administration. There was no effective way to test this possibility since the surveys contained no measure of either conservatism or strength of party affiliation. A related explanation is based on the premise that people of higher status, being generally better-educated and informed, are more likely to express opinions consistent with their basic political premises.[46] The major difficulty with this explanation is that higher status Democrats failed to show the expected parallel tendency toward greater trust—if anything, they too tended to be less trusting. In addition, other analyses indicated that differences between Democrats and Republicans (in level of Distrust) were not substantially greater at higher as opposed to lower levels of social status—in general, marked differences existed even at lower levels.

A third possibility is that higher-status, better-informed Republicans were more influenced by the speeches of Senator McCarthy who, at this time, was expressing themes similar to those contained in the Distrust syndrome.[47] Though McCarthy was not yet well known in 1951–52, he would probably have been best known to better informed Republicans. Even at the height of his popularity his major support came from Republicans.[48] It was not possible to test this explanation, since the surveys contained no measures of familiarity with, or support for, McCarthy. Table 5 does show, however, that information was the strongest correlate of Distrust among Republicans.[49]

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zero-order associations were used in the control analysis. Race was dropped because blacks were too few in number and too heavily concentrated in the Democratic Party. Controlling for political affiliation had no effect on the association between Interventionism and SES.

[46] For discussions of this premise, see: Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David Apter, (New York: Free Press, 1964), 206–261; Gamson and Modigliani, "Knowledge and Foreign Policy Opinions," pp. 190–197; and David O. Sears, Political Behavior, in *Handbook of Social Psychology*, ed. Gardner Lindzey and Elliot Aronson (Reading, Mass.: Addison-Wesley, 1969), 2nd edition, Vol. 4, 348–354.

[47] This explanation was suggested by Philip E. Converse (personal communication).

[48] Nelson W. Polsby, "Toward an Explanation of McCarthyism," in *Politics and Social Life*, ed. Nelson W. Polsby, Robert A. Dentler, and Paul A. Smith (Boston: Houghton-Mifflin, 1963), pp. 809–824.

[49] If we assume that the Distrust syndrome was capitalized on even if not created by McCarthy, our data bear directly on the continuing debate over the sources of McCarthy's support. Lipset and other theorists have argued that McCarthy's support was populist in nature—largely bi-partisan and concentrated among lower and middle SES sectors, especially among those subjected to status losses or status inconsistency. However, Polsby found only small associations with SES and a strong tendency for Republicans to be more pro-McCarthy. Rogin argues that McCarthy's appeal was not populist but elitist, resonating most with the traditional conservative wing of the Republican Party. He dismisses McCarthy's somewhat greater support among the less educated as a superficial endorsement of an anti-Communist crusader not based on any real support of his ideology. (See Lipset, "Three Decades of the Radical Right"; Bell, *Radical Right;* Polsby, "Explanation of McCarthyism," and Rogin, *Intellectuals and McCarthy.*) If the Distrust syndrome reflects a receptiveness to McCarthy's ideology, then the present data give strong

The final possibility to be considered here is more complicated but also broader in its implications. Recall that persons scoring high on Distrust believed not only that the government was performing quite poorly but also that administration officials were incompetent and disloyal. The latter belief is noteworthy because it indicates that Distrusters were *not* satisfied to account for the government's poor showing in terms of the complexity of the problems it faced. Instead, they blamed it on the personal shortcomings of administration officials. Put in the language of attribution theory, they tended to attribute the administration's poor performance to personal factors rather than to situational factors.[50]

Is there any reason to expect persons higher in SES to be more prone to make such attributions? A number of studies have shown that those higher in SES tend to be higher on "internal control"[51]—that is, they are more likely to attribute events in their lives to internal factors such as effort or skill, rather than to external factors such as luck or others' malevolence or benevolence. Since persons higher in SES are, indeed, in greater control of their environments, these findings are not surprising. But they can be generalized to yield the following less obvious hypothesis: people of higher SES, being less aware of the environment as a source of constraint, are more likely than those of lower SES to attribute the poor performance of *others* to internal (personal) rather than to external (situational) factors. To account for the findings of this study, it is necessary to add the assumption that this effect will be most pronounced among Republicans when the performance in question is that of a Democratic administration. In effect, party loyalty is likely to mitigate against personal attribution among higher status Democrats.

An item included on survey #315 offered an unusual opportunity to test these speculations. It asked respondents whether they felt the government was "making any real progress in its efforts to stop the spread of Communism, or not." Thirty per cent of the sample said it was not. (Naturally, this group tended to score high on the Distrust Scale—75 per cent were above the median.) This group was then asked the following open-ended question: "Why do you feel they are making no progress?" Responses to this open-ended question could be grouped under three broad headings: The situation is complicated,[52] our policy is not tough enough,[53] and our policy makers are disloyal or incompetent.[54]

According to the preceding argument, two things should be true. First, respondents attributing the lack of progress to disloyalty or incompetence should score higher on the Distrust Scale than respondents making other kinds of attributions. Second, respondents of higher social status should be more prone to make the attribution to disloyalty or incompetence than respondents of lower status. The first of these hypotheses was clearly borne out: 92 per cent of the "incompetence or disloyalty" group scored above the median on Distrust as opposed to only 72 per cent of the "not tough enough" and 66 per cent of the "complex situation" groups (gamma=.42). Data pertinent to the second hypothesis are displayed in Table 6. Although all the measures of SES consistently show that higher status respondents were more likely than lower status respondents to make the expected attribution, the statistical significance of some of these associations is rather marginal. The strongest effects are for race and education.[55] On the whole, these results must be considered impressive because the effect we are postulating must overcome a "sophistication factor": that is, better-educated respondents are more likely to have the conceptual skills and information needed to give answers referring to situation complexities. Thus, it is not surprising that answers of this type showed little tendency to decrease at higher levels of status.[56]

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support to Rogin's argument: Distrust *was* greatest among high status Republicans.

[50] For a general discussion of attribution theory, see H. H. Kelley, "Attribution Theory in Social Psychology," in *Nebraska Symposium on Motivation*, ed. David Levine (Lincoln, Nebraska: University of Nebraska Press, 1967).

[51] Several are reviewed in Julian B. Rotter, "Generalized Expectancies for Internal versus External Control of Reinforcement," *Psychological Monographs*, 80, No. 1, (1966).

[52] For example: Communism is difficult to stop, hard to deal with; our aid program is not working; we are indirectly supplying the Communists with equipment; we can't seem to reach a solution in Korea; we need to take care of people and problems here at home first.

[53] For example: they are too soft, conciliatory, or defensive in dealing with Communist countries; they don't take strong enough action against Communist groups here at home.

[54] For example: too many Communists in Washington; some officials are trying to help the Communists; officials are corrupt, selfish, playing politics, stupid, disorganized, confused, etc.

[55] The results in Table 6 are *not* a spurious consequence of the fact that Democrats are both lower in SES and less prone to be critical of administration officials. While Democrats as a whole are less likely than Republicans to see a lack of progress against Communism, those who do see it are no different than like-minded Republicans in their willingness to attribute it to disloyalty or incompetence.

[56] The "sophistication factor" may make it difficult

Table 6. Socioeconomic Status and Attribution of Poor Governmental Performance to Disloyalty or Incompetence

(Data from NORC 315, in Per Cent)[a]

			Why no progress against Communism?			
			Complex situation	Not tough enough	Disloyalty or incompetence	
SES Index	Low	(68)[b]	31	56	13	100%
	Med. Low	(81)	36	42	22	100%
	Med. High	(55)	24	47	29	100%
	High	(68)	24	43	34	100%
				Gamma = .13, p = .25[c]		
Education	0 to 11th	(215)	30	51	19	100%
	12th & over	(141)	27	39	34	100%
				Gamma = .20, p = .02[c]		
Race	Non-white	(23)	48	52	0	100%
	White	(332)	27	39	34	100%
				Gamma = .54, p = .03[c]		
Economic Level	Low	(104)	26	58	16	100%
	Medium	(189)	32	41	27	100%
	High	(60)	27	40	33	100%
				Gamma = .09, p = .10[c]		
	Marginals		29	46	25	100%

[a] Includes only respondents who felt the govt. was making "no real progress in its efforts to stop the spread of communism."

[b] Figures in parentheses are n's on which row per cents are based.

[c] Significance levels are based on chi-square.

In sum, only two of the four proposed explanations for the association between SES, Distrust, and political affiliation could be tested. Of these, only the one based on attribution theory received some support. Since the various explanations are not mutually exclusive, there is no reason why several could not have been operating simultaneously.

Demographic Composition of the Four Types. Table 7 summarizes the demographic composition of the four types. It shows that Trusting-isolationists were the lowest in SES, were the least informed, and contained the lowest proportion of males. Conversely, Distrusting-interventionists were the highest in all these categories. The remaining two groups were intermediate and fairly similar in demographic composition. Republicans were disproportionately overrepresented in the two Distrusting groups. A separate analysis that controlled for political affiliation and classified by SES levels yielded the following results: Republicans above the

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to demonstrate the proposed association between status and attribution even when the question is not put in an open-ended manner as it was here. In many instances, to blame events on the personal shortcoming of actors is obviously more simple-minded than accounting for them in terms of environmental factors or more complex interactions. For example, Gallup asked the following question of a national sample in April 1963: "Who do you think is to blame for the present condition in which Negroes find themselves: white people or the Negroes themselves?" There was only a slight positive association between occupational status and propensity to hold whites responsible (Phi = .13, p = .05). Though counter to the prediction from the attribution argument, the weakness of the association is still noteworthy since the prediction is here pitted against both conceptual sophistication and racial prejudice. See Michael M. Ross, "Resistance to Social Change in the Urban North" (Unpublished Doctoral Dissertation, Harvard University, Department of Social Relations, 1972).

A study by Sherwood and Nataupsky is especially significant because it controls for conceptual sophistication by dealing only with a sample of college-educated researchers (most with Ph.D.'s), all of whom had conducted research on Negro-White differences in performance on I.Q. tests. They found that researchers of higher SES background were more likely than those of lower SES background to conclude that Negro-White differences in I.Q. were due to innate rather than environmental factors. (John J. Sherwood and Mark Nataupsky, "Predicting the Conclusions of Negro-White Intelligence Research from Biographical Characteristics of the Investigator." *Journal of Personality and Social Psychology,* 8 (January, 1968) pp. 53–58.)

Table 7. Demographic Composition of the Four Types[a]

(Data from NORC 317 in Per Cent)[b]

		Isolationists		Interventionists		Full Sample Marginals[c]	
	n =	Trusting	Distrusting	Trusting	Distrusting		
		(166)	(166)	(196)	(205)		
SES Index	Low	47	26	17	14	26	
	Med-Low	31	26	29	23	26	Gamma = .36
	Med-High	16	22	28	24	24	Phi = .35
	High	6	26	26	39	24	
Education	0 to 11th	87	61	59	41	61	Gamma = .48
	12th & over	13	38	41	59	39	Phi = .34
Economic Level	Low	51	29	25	19	31	Gamma = .34
	Medium	42	54	55	53	52	Phi = .29
	High	7	17	20	29	17	
Information	Low	60	37	24	9	33	Gamma = .60
	Medium	40	63	76	91	67	Phi = .40
Sex	Female	61	49	46	44	51	Gamma = .17
	Male	39	51	54	56	49	Phi = .13
Political Affiliation	Republican	20	45	20	42	30	Gamma = −.16
	Independent	10	23	16	20	20	Phi = .33
	Democrat	70	32	64	37	50	

[a] Upper and lower thirds of Interventionism and Distrust Scales are used to create four types—middle thirds are dropped.

[b] Columns in each table sum to 100%.

[c] Measures of association are based on the figures and sample sizes shown in the body of the Tables.

median on the SES index were heavily Distrusting-interventionists (50 per cent), while those below the median were fairly evenly distributed across all four groups. Democrats above the median in SES were largely Trusting-interventionists (47 per cent), while those below the median tended to fall in the two Isolationist groups (66 per cent). In general, these findings are quite consistent with the correlational analysis displayed in Table 5 which was based on the full samples.[57]

Escalation and SES. At least one puzzling feature remains in the overall picture that has been emerging: if Distrusting-interventionists and Trusting-isolationists were the most widely separated groups on measures of SES (Table 7), and if the former were the most proescalation while the latter were the most antiescalation (Table 4), it may seem surprising that an overall positive association failed to emerge between SES and support for escalation (Table 1). Part of the answer lies with the remaining two groups: Distrusting-isolationists were slightly lower in SES than Trusting-interventionists but more prone to support escalation. Beyond this, it should be remembered that Tables 4 and 7 are both based on subsamples of respondents who scored either quite high or quite low in Interventionism and Distrust. Hence, these tables exaggerate the overall associations of Distrust and Interventionism with both SES and escalation. Recall, for example, that Distrust had a notable association with SES only among Republicans (Table 5), who constitute about 30% of the total sample. Similarly, Interventionism and even Distrust had only moderate associations with escalation over the entire sample—for example, Distrust had median gamma coefficients of .29 with the "bomb China" item, .34 with the "aid Formosa" item, and .29 with the "break off talks" item. In short, the zero-order correlations among these variables are not sufficiently strong to warrant the expectation of a notable association between SES and support for escalation.

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[57] Table 7, like Table 4, is based only on that portion of the sample that scored either high or low on both Interventionism and Distrust.

Still, the association might well have been stronger than it was. In order to pursue the matter further, analyses were undertaken to examine whether the military policy preferences of the four types might have differed at different levels of SES. This analysis revealed very few differences that held up reliably across different surveys. There was, however, a small but consistent tendency for persons in the two Trusting groups to be more opposed to escalation the higher they were in SES. This trend contributed to weakening the association between SES and escalation—it added enough higher status persons to the antiescalation camp to offset the general tendency of higher status persons to be more distrusting, and hence, more proescalation.

Summary and Conclusions

The key finding of this study was that public opinion on military policy during the Korean War could not be organized in terms of a single "hawk to dove" dimension. Sentiment to escalate and sentiment to disengage did not emerge as opposite poles of a single continuum. Instead, they emerged as independent paths leading away from current policy. Those who favored one of these paths were not necessarily inclined to reject the other—sentiment to escalate and sentiment to disengage had only a slight inverse association. Thus, a notable segment of the public had "intransitive" preference orderings wherein they preferred both escalation and disengagement to current policy.

Opposition to disengagement came predominantly from a sector of the public that was internationalist and interventionist in outlook. This segment supported all forms of foreign aid and tended to accept the basic premise of American foreign policy in this period: That the United States should be ready and willing to help out wherever the values of the "Free World" seemed in jeopardy. Not surprisingly, those most imbued with this "enlightened" orientation tended to be from the higher strata of society—white, better educated, better informed, and more wealthy. Conversely, those nearer the periphery of the society, with more parochial outlooks and less exposure to the official values of American foreign policy, tended to be isolationist in outlook and constituted the bulk of support for military disengagement. Women also tended to fall in the isolationist-disengagement camp, presumably because their predominant orientation to the family environment isolates them from the mainstream of national affairs much as does low SES.[58] In addition, of course, more peaceful attitudes are consistent with the traditional female sex role which, among other things, places a lower value on active intrusions into new and distant environments.

Those with interventionist orientations were only moderately inclined to support escalation. The bulk of proescalation sentiment came from a sector of the public that harbored a marked distrust of the Truman administration. This group not only felt that the administration was dealing inadequately with the problems confronting it, but also believed that some officials were incompetent and others actually disloyal. Predictably enough, such attitudes were much more prevalent among Republicans than Democrats. More surprisingly, they were most prevalent among Republicans of higher SES. A number of plausible explanations for this result were set forth earlier and will not be reviewed here. In general, those with trusting attitudes toward the Truman administration formed the bulk of support for the then current policy as against escalation.[59]

Since those with Interventionist outlooks had only the slightest tendency to be more trusting of the Truman administration, it proved useful to conceive of the public as divided into four segments—as regards the Korean War—each with a characteristic outlook on the war, and each with a unique preference ordering among military alternatives. Trusting-isolationists were oriented to getting out of the war and favored disengagement while opposing escalation; Distrusting-isolationists were oriented to getting the war over with and favored both escalation and disengagement to current policy; Trusting-interventionists were oriented to carrying on with the war effort and favored current policy while opposing both disengagement and, to a lesser extent, escalation; finally, Distrusting-interventionists were oriented to winning the war and favored escalation while rejecting disengagement.

How useful this framework will prove in understanding public opinion on Vietnam remains

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[58] Women tend to score much lower than men on measures of political knowledge even when education is controlled.

[59] Negroes were a markedly dovish sector of the public during both the Korean and Vietnam wars. Verba et al., "Public Opinion and the War in Vietnam," p. 331, attribute their Vietnam attitudes to a disenchantment with, and lack of commitment to, American society. While this explanation is certainly plausible, especially in view of the civil rights turmoil that coincided with the Vietnam War, the Korean data, at least, can be explained simply in terms of the social characteristics of Negroes. Not only were Negroes on the periphery of the society, and hence isolationist, but they were also heavily loyal to the Democratic party, and hence, Trusting.

to be seen. The author is now in the process of extending his analysis to this period, seeking to identify comparable segments in the public. In the meantime, certain important parallels between these historical periods can be noted. First, Vietnam opinion shows a similar lack of clear inverse association between sentiment to escalate and sentiment to disengage. Verba et al. report only a —.37 product-moment correlation between their Escalation and De-escalation scales.[60] Had their De-escalation scale focused more exclusively on disengagement, rather than including items about coalition government, free elections, and a U.N. supervised truce, the correlation might have been still weaker.

We have already reviewed the demographic correlates of support for withdrawal and noted their similarity to those found during the Korean War. Taken together, these findings point rather clearly to the same Isolationist—Interventionist dimension found in the Korean data.

The issue of political distrust and support for escalation is more complex. It is clear that the Johnson administration suffered a loss of trust quite comparable in magnitude to that experienced by the Truman administration.[61] But the quality of this distrust was different. Rather than centering on issues of internal subversion or being "soft on Communists," it centered on a "credibility gap"—on a belief that the administration was being less than candid in its reports to the public. Presumably, this form of distrust, like its counterpart in Korea, could be expected to produce a disenchantment with current military policy, but it is not clear whether it should lead to support for escalation.[62] The consequences of the war's being passed on to a Republican administration are even more a matter of conjecture.[63]

Perhaps the general task of greatest importance for future research is that of mapping the cognitive orientations of different segments of the public. One need not assume that these orientations will approach the consistency of well-formed ideologies to admit their usefulness in understanding military policy preferences. Failure to ascertain these orientations merely tempts us to superimpose our own biased conjectures.

At the outset of this study, for instance, the author considered it a foregone conclusion that the public tended to evaluate military alternatives on a "soft-to-tough" dimension. Only repeated failures to make sense of the data forced him to realize that different segments of the public were evaluating military policies from very different vantage points. Interventionists, for example, probably *were* oriented to a "soft-tough" dimension, in the sense of being concerned with the force needed to attain desired international objectives. Yet, even within this group, many were probably more concerned with supporting or opposing the administration than with making strategic judgments. Isolationists, on the other hand, seemed primarily oriented to the costs associated with foreign involvements and probably had little appreciation of international objectives or the force needed to attain them. While some did favor tougher policies, this stance more nearly reflected a desire to terminate the involvement quickly than a concern with achieving political objectives.[64]

It is these sorts of broader cognitive orientations that need to be mapped and further explored. New techniques of multidimensional scaling should prove useful in this endeavor,[65] but the groundwork must be laid through open-ended questions. We must beware of relying prematurely on fixed-alternative items, for we run too great a risk of imposing our own cognitive maps on the respondent. In losing the respondent's perspective, we lose precisely that information that ultimately will prove most valuable in understanding public opinion on military policy.

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[60] Verba et al., "Public Opinion and the War in Vietnam," p. 323.

[61] *Gallup Opinion Index*, Report 56, 1970, pp. 8–16.

[62] The problem is further complicated by the fact that the Johnson administration took a number of escalatory steps thus blurring the distinction between escalation and current policy. Presumably such steps would receive their greatest support from the politically distrusting prior to their implementation, but should be adopted by the politically trusting once implemented.

[63] The most interesting questions here center on the reversibility of trusting and distrusting attitudes. Do Republicans now become trusting while Democrats become distrusting, or are the dissidents likely to remain so until the war is settled? Another important issue concerns the role of Opposition leaders. If such leaders tend to favor disengagement rather than escalation, will this encourage distrusting persons to have similar preferences, or will the positive association between distrust and escalation continue to hold?

[64] For data bearing on the greater cost-orientation of grade-school educated, as opposed to college-educated, opponents of the Vietnam War, see Albert H. Cantril, "The American People, Vietnam and the Presidency," paper delivered at the Sixty-sixth Annual Meeting of the American Political Science Association, Los Angeles, California, September, 1970, Appendix, Part D.

[65] See John P. Robinson and Robert Hefner, "Multidimensional Differences in Public and Academic Perceptions of Nations," *Journal of Personality and Social Psychology*, 7 (November, 1967) 251–259; and Myron Wish, Morton Deutsch and Lois Biener, "Difference in Conceptual Structures of Nations: An Exploratory Study," *Journal of Personality and Social Psychology*, 16 (November, 1970) 361–373.

Policy Voting and the Electoral Process: The Vietnam War Issue*

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Most theories which hold that the preferences of the public affect policy making are predicated on the assumption that elections link opinions with policies. Electoral links, in turn, are thought to depend upon the ability of citizens to take account of their policy preferences when deciding how to vote. For that reason "issue voting" has been a focus of research by scholars concerned with empirical democratic theory.

Previous research on voting behavior in America has not, on the whole, been kind to democratic theory. It has supported two main propositions: first, that policy voting is quite rare; and second, that the infrequency of policy voting results from the shortcomings of citizens. Voters' lack of attention, information, and involvement with the issues saps their ability to choose candidates on the basis of policy preferences.[1] In short, voters are incapable of policy rationality.

Before final judgment is passed upon the capacities of the American voter, however, several challenges to one or both of these propositions ought to be tested.

One challenge alters the grounds of discussion. V.O. Key has argued that however little prospective *policy* voting may occur, the requirements of some democratic theories may be met if retrospective evaluations of governmental *performance* takes place. He claimed that such evaluations are common.[2]

A second challenge points out that different members of the population are concerned with,

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This paper was originally presented at the 1971 annual meetings of the American Political Science Association. It continues a line of investigation of which earlier results were reported in Richard A. Brody and Benjamin I. Page, "Issues in an Election: Vietnam and Presidential Voting, 1968," paper delivered at the International Political Science Association meetings in Munich, September 1970; and in Richard A. Brody, Benjamin I. Page, Sidney Verba, and Jerome Laulicht, "Vietnam, the Urban Crisis, and the 1968 Presidential Election: A Preliminary Analysis," paper delivered at the American Sociological Association meetings in San Francisco, September 1969.

The authors are grateful to Sidney Verba for his comments and suggestions.

[1] The literature bearing on these points is extensive. It includes Paul F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice: How the Voter Makes Up His Mind in a Presidential Campaign* (New York: Columbia University Press, 1944), which emphasizes social factors in voting; Bernard Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting: A Study of Opinion Formation in a Presidential Campaign* (Chicago: University of Chicago Press, 1954), which explores social influences on the vote, and misperceptions of candidates' issue positions; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), which establishes the primacy of party identification, and shows the weakness of opinions and perceptions about issues; Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent* ed. David E. Apter (New York: Free Press, 1964), pp. 206–261, which demonstrates that opinions on policy lacked any simple ideological structure, and were unstable over a four-year period; and Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March 1966), 19–28, which shows that issues have been less important than candidate images as "short term forces" affecting electoral outcomes.

The late V. O. Key manfully tried to find evidence of an issue-oriented "responsible electorate" among those who switched their votes from one election to the next, but his effort was beset by methodological difficulties. Respondents' recollections of their votes four years previously, which Key used to identify switchers, are not very reliable; the issues upon which he found agreement between opinions and direction of switch were broadly phrased judgments about governmental performance, which are particularly subject to rationalization by voters who switch for other reasons. V. O. Key, Jr., *The Responsible Electorate* (Cambridge, Mass.: Harvard University Press, 1966).

[2] V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Knopf, 1961), pp. 458–480. Gerald H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964," *American Political Science Review*, 65 (March 1971), 131–143, provides some evidence of electoral punishment in response to bad economic conditions.

and have strong opinions about, different policy areas; policy voting may be widespread, but the relevant area of policy may vary from one voter to the next.[3]

A third challenge notes that much of the strongest evidence against issue voting derives from the politically tame 1950s; perhaps citizens have subsequently become more aware of the political world around them, more assertive in their reactions to it, and more willing to act on the basis of their opinions.

Finally, a fourth challenge is suggested by such diverse observers as Anthony Downs and George Wallace: perhaps there isn't "a dime's worth of difference" between Democratic and Republican candidates.[4] If rational campaigning dictates a side-by-side position for the candidates on the issues, then voters do not have the option of choosing between them on policy grounds—even if, given a real choice, they would be perfectly capable of doing so. Further, if candidates equivocate and hedge their position on the issues, voters should not be blamed for failing to perceive their positions clearly, or failing to vote on policy grounds.[5]

At present we are concerned only with the third and fourth challenges.[6] In the context of a single, highly salient issue we will consider these questions: First, was there a strong relationship between policy preferences and the vote? A positive answer to this question would impeach the generality of findings from studies which were carried out in the 1950s.

Second, were citizens able to vote their preferences? Did they have opinions; could they perceive what policy positions candidates took; could they take their opinions and perceptions into account in deciding how to vote? Positive answers to these questions would tend to overturn the proposition of incapacity, but not necessarily the proposition that policy voting is rare.

Finally, if we find that citizens were able to vote their preferences but did not do so, we will inquire into candidate behavior to see whether it depressed the extent of policy voting.

The Study

The 1968 presidential election, coming at the height of the war in Vietnam, offered an opportunity to examine these questions. At the outset of 1968, nearly 500,000 American troops were in Vietnam; at least 20 billion dollars per year were being spent on the war; and each week about 100 Americans—and countless Vietnamese—were being killed. The war had a direct impact upon the lives of servicemen, their families, and friends. It affected many others indirectly, through rampant inflation and curtailment of domestic governmental spending.

Most Americans were thoroughly sick of the war. By late March, 1968, nearly two-thirds of the people disapproved of the way President Johnson was handling Vietnam, and a plurality thought it had been a mistake to send U.S. troops to fight there.[7] Dissent was loud on university campuses, in the U.S. Senate and even on Wall Street. A political candidate, Eugene McCarthy—followed by Robert Kennedy—offered a serious challenge to the incumbent President for the nomination of his own Democratic Party. To most observers it seemed that if Americans ever voted on the basis of policy preferences, they would vote their opinions on Vietnam in 1968.

Beginning in February 1968, we commissioned a series of nationwide opinion surveys, designed primarily to discover how Vietnam entered into public perceptions and evaluations of presidential candidates, and into the final voting decision.[8] In addition, the Survey Research Center of the University of Michigan, carrying out its usual election study for the Inter-University Consortium for Political Research, agreed to include some of our key items in its questionnaire. The November 1968 findings reported in this paper come from our analysis of the SRC data; findings concerning previous months are drawn from the ORC/IEP surveys.

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[3] The existence of "issue publics" is demonstrated in Converse, "Belief Systems," pp. 245–246. David E. RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June 1971), 389–400, finds a strong correspondence between voting choices and the party whose stand is preferred on salient issues; as he points out, however (p. 395), some of this correspondence may result from rationalization.

[4] Electoral competition models which predict party convergence are set forth in Harold Hotelling, "Stability in Competition," *Economic Journal*, 39 (March 1929), 41–57; Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957), pp. 114–141; and Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June 1970), 426–448.

[5] Stanley Kelley, *Political Campaigning* (Washington, D.C.: Brookings, 1960) finds considerable evidence of candidate ambiguity. Theoretical arguments predicting ambiguity are given in Downs, pp. 135–137; and in Kenneth A. Shepsle, "The Strategy of Ambiguity: Uncertainty and Electoral Competition," *American Political Science Review*, 66 (June, 1972), pp. 555–569.

[6] In the IEP project as a whole, we are studying a variety of questions about the issue-related behavior of citizens and candidates, including the other challenges mentioned above.

[7] *Gallup Opinion Index*, April 1968, p. 3, 14.

[8] The surveys were conducted in February, June, August, and November 1968 by the Opinion Research Corporation (Princeton, New Jersey) for the IEP project. In addition to the present authors, the project includes Jerome Laulicht and Sidney Verba.

In each survey, respondents were asked to indicate what Vietnam policy they preferred, on a seven-point rating scale. The scale was introduced by a prologue in which it was stated that opinions about Vietnam differed; some people favored all necessary measures to achieve a complete military victory; others favored immediate American withdrawal from Vietnam; and others, of course, had opinions somewhere in between. The interviewer then displayed a printed card with the numbers 1 through 7 arrayed at equal intervals along a line. Point number 1 was labeled "immediate withdrawal," and point number 7 "complete military victory." Other points were designated only by numbers. Respondents were then asked where on this scale they would place themselves, or whether they had no opinion.

Evidence from previous research indicated that the Vietnam preferences of most of the population were jointly single-peaked. That is, policy options concerning the level of American troops in Vietnam, bombing of North Vietnam, coalition government and the like could be arrayed along a single dimension running from de-escalation to escalation.[9] When a respondent chose a single point as his mosr preferred policy, one could infer that alternative policies would be progressively less favored by him as they were located farther from his position. Examination of attitudes in 1968 has confirmed that Vietnam policy preferences continued to be unidimensional, and that our rating scale measured them satisfactorily.[10]

In addition to expressing their own opinions on the seven-point scale, respondents in each survey were asked to locate each presidential candidate's stand on the same scale. They also gave their evaluations of each candidate, on a scale ranging from "very unfavorable," and "not very favorable," through neutral, to "fairly favorable" and "very favorable."[11]

Candidate Similarity and Policy Voting

It is quite difficult to assess precisely how much effect policy preferences have upon voting. In order to do so, it is probably necessary to calculate citizens' perceptions of similarity or dissimilarity between their own opinions and candidates' stands on an issue; to ascertain the relationship between these "perceived issue differences" and the vote; to estimate the extent of issue-oriented evaluation, disentangling that process from the confounding effects of rationalization (perceiving a favored candidate's position as being the same as one's own, or shifting one's opinion to conform with the position of a favored candidate); and, finally, to calculate net impact on the electoral outcome in terms of the average amount of issue difference from the candidates which is perceived by the population, and the average effect of a single unit of difference. It is probably correct to say, therefore, that the impact of issue voting has never been adequately measured.[12]

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[9] In 1967, specific Vietnam items formed a satisfactory Guttman scale, with CR = .88 and CS = .66. Richard A. Brody and Sidney Verba, "Hawk and Dove: The Search for an Explanation of Vietnam Policy Preferences," *Acta Politica*, forthcoming. In an earlier paper, based on data from 1966, it was reported that opinions fell into *two* imperfectly related scales, one expressing attitudes toward escalation, and the other attitudes toward de-escalation. Sidney Verba, Richard A. Brody, Edwin B. Parker, Norman H. Nie, Nelson W. Polsby, Paul Ekman, and Gordon S. Black, "Public Opinion and the War in Vietnam," *American Political Science Review*, 61 (June 1967), 317–333, material cited is on pp. 320–321. Reinterpretation of those data suggests that a single escalation/de-escalation dimension might have existed even as early as 1966.

[10] In 1968 we did not have enough policy items to replicate the Guttman scaling analyses of 1966 and 1967. We found, however, that opinions on the Vietnam rating scale correlated quite highly with opinions concerning bombing, troop levels, and three general policy alternatives.

[11] In the SRC survey, evaluations were measured on a 100-point "thermometer" which yielded almost precisely the same levels of evaluation and the same relationships with other variables as the five-point ORC/IEP favorability scale. Benjamin I. Page, "Party Loyalty and the Popularity of Presidential Candidates" (unpublished ditto, Feb., 1972). For a discussion of the candidate thermometer, and some findings based on it, see Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review*, 64 (December 1970), 1167–1185.

[12] The approach to policy voting which is sketched here would blend two theoretical and two methodological traditions. A formal model of issue voting drawn from the electoral competition literature (see Davis et al.) would be supplemented by psychologists' understanding of the processes of projection and persuasion. Simultaneous equation techniques from econometrics would be combined with Donald Stokes's method of computing net effects from means and estimated slopes. (See Stokes, "Some Dynamic Elements of Contests for the Presidency," pp. 27–28.) See Richard A. Brody and Benjamin I. Page, "Comment: The Assessment of Policy Voting," *American Political Science Review*, 66 (June, 1972), pp. 450–459.

Our early efforts to perform such an analysis with respect to the Vietnam issue are reported in Brody et al., "Vietnam, the Urban Crisis and the 1968 Presidential Election" and in Brody and Page, "Issues in an Election." It will not be possible to reach a precise conclusion about the impact of the issue without completing estimation of a nonlinear three-equation

It is rather easy, on the other hand, to discover when a given issue has little or no impact. The more traditional mode of analysis, a simple cross-tabulation of vote by policy preferences, reveals whether those who held one opinion voted differently, on the average, from those who held a contrary opinion. If there is little or no relationship between opinion and vote, one can conclude that there is little policy voting.[13]

Using this method, we examined the relationship between votes for Richard Nixon and Hubert Humphrey in 1968, and Vietnam policy preferences, as measured on our seven-point scale and by a similar three-alternative item. It is apparent that Vietnam policy preferences did not have a great effect on voting for the major party candidates in 1968.[14] (See Table 1.)

"Hawks" tended to vote for Nixon in somewhat greater proportion than "doves" did. This tendency was statistically significant, and was larger than such relationships for many other issues in this and other elections. Yet, considering the enormous impact of the war on American society, a difference of 10 to 15 per cent in candidate choices by extreme hawks and extreme doves does not seem very impressive. Even less impressive is the fact that Vietnam opinions could account for only a minuscule fraction—between 1 and 2 per cent—of the variation in voting behavior.[15]

We are not able, then, to challenge as time-bound the established finding of little issue voting. Times may have changed between the apathetic 1950s and the militant 1960s and 1970s, but if so, this change was not reflected in Vietnam policy voting. Voters did not treat the 1968 election as a referendum on Vietnam policy.[16]

We are left, however, with the question of why such a potent issue had so little effect on voting. One possibility—that Americans simply didn't have opinions about the war—can be rejected at the outset. Only 7 per cent failed to express an opinion in response to the three-alternative item, and only 5 per cent failed to locate themselves on the seven-point Vietnam scale. Further, we can reject the explanation that people failed to attach some minimal level of intensity to their opinions. When asked what they felt was the most important problem the government in Washington should try to take care of, nearly half the people spontaneously mentioned Vietnam—far more than mentioned any other issue or cluster of issues. Most of those who cited Vietnam said they were "extremely" concerned about the problem. This

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model. As is argued below, however, such a degree of precision is not needed for our present purpose.

Empirical work with perceived issue differences (or "distances," or "proximities"), in addition to our own, includes Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review*, 63 (December 1969), 1106–1119; and papers by David M. Kovenock and James W. Prothro of the Comparative State Elections project.

[13] The traditional technique has one great advantage over an analysis which uses perceived issue differences: the opinion-vote relationship is not inflated by projection, in which respondents tend to imagine that the positions of favored candidates are the same as their own opinions. It has two major defects, however: (1) It may *understate* the importance of policy voting as a psychological process among individuals. Two people with the same policy preferences can receive different information (even different but *correct* information) about the stands of candidates; even if they both vote in accord with their opinions and their information about candidates' positions, they may vote for different candidates and thereby reduce the apparent relationship between opinion and vote. As we argue above, precise assessment of policy voting requires explicit consideration of perceptions. (2) It may *overstate* the amount of policy voting: some people whose opinions and votes correspond may have brought opinions into line with intended vote, rather than vice versa.

In most cases the second defect is probably the more serious. Since it inflates the relationship found in the table, however, it cannot impeach a finding of little or no relationship. The first defect, which can lead to an underestimate of policy voting, requires that even a claim of no relationship be qualified, or that it be supported by additional evidence.

[14] This claim requires evidence that the weak relationship in the table does not conceal policy voting by those with differing perceptions. In previous work, employing the concept of issue distance, we also found the effects of Vietnam policy preferences to be relatively small. Brody et al., "Vietnam, the Urban Crisis, and the 1968 Presidential Election;" Brody and Page, "Issues in an Election." Since those findings inflated the apparent impact of Vietnam, by accepting all opinions and perceptions as causally prior to the vote, they support the present finding *a fortiori*. Further evidence is presented in the next section.

[15] According to the coefficient of determination (r^2), in a linear regression, variations in opinion could account for only 1.0 per cent or 1.2 per cent of the variance in voting. According to the correlation ratio (eta), which better reflects the nonlinear relationship, variations in preference could account for 1.5 per cent of 1.8 per cent of the variance in the vote.

[16] This conclusion must be distinguished sharply from the assertion that Vietnam opinions had little effect upon the 1968 election. We are concerned here only with preferences about future policy, not with evaluations of past performance in Vietnam. Further, we are concerned only with the effects of these attitudes on voting choices between Humphrey and Nixon; we do not consider their effects on voting for Wallace, abstentions from voting, or decisions whether or not to give time and money to the campaign, to say nothing of effects on the launching and subsequent fate of the prenomination candidacies of Eugene McCarthy and Robert Kennedy, or the withdrawal from candidacy of President Johnson. From this broader perspective the impact of Vietnam opinions seems to have been profound.

Table 1. Vietnam Policy Preferences and the Major Party Vote, 1968

Vote	Opinion on Vietnam Pull Out Entirely	 Keep Soldiers, Try to End Fighting	 Take Stronger Stand, Even if it Means Invading North Vietnam
Nixon	51% (88)	47% (174)	62% (180)
Humphrey	49 (86)	53 (194)	38 (109)
	100 (174)	100 (368)	100 (289)

Note: Tau b = .10 Correlation ratio (eta) = .018.
Pearson correlation (r) = .10 r^2 = .010.

Vote	Opinion on Vietnam Immediate Withdrawal 1	 2	 3	 4	 5	 6	 Complete Military Victory 7
Nixon	42% (46)	51 (38)	51 (39)	52 (139)	59 (60)	64 (47)	59 (83)
Humphrey	58 (63)	49 (36)	49 (37)	48 (130)	41 (42)	36 (26)	41 (57)
	100 (109)	100 (74)	100 (76)	100 (269)	100 (102)	100 (73)	100 (140)

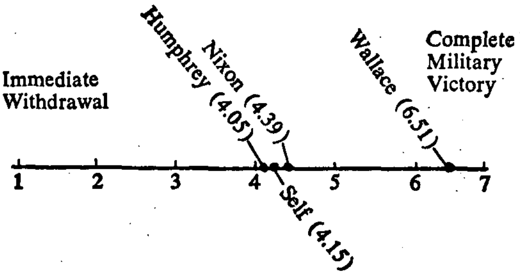
Note: Tau b = .10 Correlation ratio (eta) = .015.
Pearson correlation (r) = .11 r^2 = .012.

was not a case of "nonattitudes," or weakly held opinions.

Instead, the explanation seems to lie with public perceptions of where the candidates stood on Vietnam.

The average American perceived Nixon and Humphrey as standing very close together on the escalation/de-escalation dimension. With seven distinct points to choose from, in fact, the average perceptions of Nixon's and Humphrey's positions on the Vietnam scale were only one-third of one point apart.[17] (See Figure 1.) The perceived similarity between Nixon and Humphrey is underscored by the fact that George Wallace was seen, on the average, as fully two points more hawkish than Nixon.

Figure 1. Average Perceptions of Candidates' Vietnam Positions, November 1968



Entries are the median ratings of respondents' own opinions and their perceptions of candidates' positions on the seven-point Vietnam scale.

Average perceptions like those displayed in Figure 1 smooth over the considerable variation in perceptions by individuals. An examination of perceptions at the individual level buttresses this finding, however. Of those who assigned both Nixon and Humphrey to positions on the seven-point scale, well over half saw no difference at all, or only a difference of one point, between the two.[18] For these citizens, there was little to choose between Nixon and Humphrey in term of Vietnam policy. As they saw it, either candidate would act about equally in accord with—or equally contrary to—their wishes on Vietnam; even if Vietnam policy was extremely important to them, they had to ignore the issue and vote on other grounds.[19]

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[17] This difference is statistically significant, according to a one tailed *t*-test of the means, but substantively it is quite small.

[18] Fifty-seven per cent saw little or no difference. Thirty per cent saw no difference at all: they placed Nixon and Humphrey at exactly the same point on

In order to explain the widespread failure to perceive a difference between the candidates, it is not necessary—at least in this case—to postulate that people were unable to perceive clearly. The candidates' campaign speeches reveal that there was, in fact, rather little difference between Nixon's and Humphrey's stated positions on Vietnam policy.[20]

We examined the texts of some 156 speeches and statements by Nixon, and of 120 by Humphrey, including all of their major addresses.[21] Neither candidate devoted much attention to the specifics of Vietnam policy, but in each case it was possible to find a few fairly extensive statements on the subject. Humphrey's most detailed pronouncements were made in his nationally televised address from Salt Lake City on September 30.[22] Nixon's came at the Republican convention,[23] and on October 7, in a question and answer session for United Press International editors and publishers.[24] These statements, together with comments made on other occasions, formed reasonably coherent positions; both candidates remained consistent through the rest of the campaign.[25]

Both candidates agreed closely about how many American troops should be maintained in Vietnam: they favored a gradual reduction, contingent on their replacement by South Vietnamese. Nixon opposed "military escalation," and favored "small unit actions" rather than search and destroy operations.[26] He opposed any effort at "conquering North Vietnam."[27] At the same time, however, he considered it "dishonorable . . . simply to pull out,"[28] and counseled against "precipitate withdrawal."[29] He strongly urged that the South Vietnamese should be trained and equipped: "As they are phased in, American troops can—and should—be phased out."[30] He promised to pursue this de-Americanization "more vigorously."[31] On other occasions, however, he indicated that there was no great hurry; U.S. forces should remain at their current level until there was "a change in the enemy's attitude,"[32] or there was "clear indication that the enemy is ready to negotiate."[33]

Humphrey, too, opposed "military escalation"[34] or an invasion of North Vietnam;[35] but he also opposed "unilateral withdrawal."[36] Like Nixon, he advocated what came to be known as "Vietnamization" of the war: he favored reduction of American combat forces "as the South Vietnamese are able to carry a greater share of their own burden. . . ."[37]

Neither candidate specified when withdrawals should begin, what pace they should follow, or whether a residual force would remain. The chief difference was that Humphrey expressed a little more certainty that they should start soon. "I would take the risk that the South Vietnamese would meet the responsibilities they say they are ready to assume in their own self-defense . . . I would sit down with the leaders of South Vietnam to set a specific timetable by which American forces could be systematically

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the scale. Similarly, in response to another SRC item, 33 per cent replied that they saw no difference between the two *parties* with respect to three general Vietnam policy alternatives.

[19] These people did not in fact vote their Vietnam preferences. Among them, the relationship between opinion and vote was irregular.

[20] We do not assume that the candidates' speeches reflected their true beliefs or intentions. They did, however, represent the best information about those intentions which was available to the public.

[21] Humphrey's speeches were kindly provided by Raymond Wolfinger, John Stewart, and the Democratic National Committee; Nixon's were provided by Representative Charles Gubser and the Republican National Committee. The texts of some of Nixon's speeches are reprinted in *Nixon Speaks Out* (New York: Nixon-Agnew Campaign Committee, 1968).

We have also used the *New York Times* (hereafter *NYT*) to find comments which candidates made in interviews, question and answer sessions, and the like.

[22] A transcript of this speech is printed in *NYT*, October 1, 1968, p. 33. Our citations refer to the press release.

[23] "Vietnam," Statement to the Committee on Resolutions, August 1, 1968.

[24] Portions of Nixon's answers are given in *Nixon on the Issues* (New York: Nixon-Agnew Campaign Committee, 1968), pp. 6–12.

[25] Humphrey was consistent after September 30; prior to his Salt Lake City speech he had vacillated, with a tendency to be more hawkish.

[26] "Vietnam," August 1, 1968.

[27] Television interview, Dallas, October 11, 1968, p. 13. This and subsequent page numbers refer—unless otherwise noted—to official press releases and transcripts.

[28] UPI Conference, October 7, 1968, in *Nixon on the Issues*, p. 6.

[29] Television interview, Dallas, October 11, 1968, p. 13.

[30] "Vietnam," August 1, 1968.

[31] Statement, Miami, September 30, 1968, p. 1.

[32] *NYT*, September 26, 1968, p. 1.

[33] Television interview, Seattle, September 24, 1968, p. 2.

[34] Remarks to Liberal Party Executive Committee, New York, August 17, 1968, p. 2.

[35] *NYT*, July 8, 1968, p. 31.

[36] Remarks to Liberal Party, p. 2.

[37] Remarks, Toledo, September 23, 1968, p. 3.

reduced while South Vietnamese forces took over more and more of the burden."[38] "If the South Vietnamese army maintains its present rate of improvement, I believe this will be possible next year. . . ."[39]

On the question of bombing North Vietnam, too, the stated differences between Humphrey and Nixon were slight. Nixon thought that the bombing of North Vietnam (which had been limited by President Johnson to the southern portion of that country) was at "about the right level" and should be continued at that level for some time.[40] He opposed raising the level of bombing, or using nuclear weapons, or conducting "area bombing" of nonmilitary targets in North Vietnam.[41] On the other hand, he also opposed an unconditional halt to bombing, calling it the highest trump card in our negotiators' hands, which should not be played unless we were to get in return from the North Vietnamese "some kind of evidence, some move on their part, that they would de-escalate their assaults on American men." He declared that "we should stop the bombing when whoever is president is convinced it will result in less American lives lost. . . ."[42]

Humphrey seemed to take a different tack when he asserted that "(a)s President, I would stop the bombing of the North as an acceptable risk for peace. . ."[43] But he did not advocate an unconditional halt, and in fact he imposed conditions on North Vietnam which were quite similar to Nixon's requirement of "some evidence" of de-escalation: "before taking action" he would "place key importance on evidence—direct or indirect, by deed or word—of Communist willingness to restore the demilitarized zone between North and South Vietnam. If the Government of North Vietnam were to show bad faith, I would reserve the right to resume the bombing."[44] The similarity with Nixon's position was increased when an associate of Humphrey's explained that restoration of the D.M.Z. was "only one of the kinds of actions" that might be taken by North Vietnam.[45] Humphrey and Nixon differed more in tone than in substance, with Humphrey expressing greater confidence that acceptable conditions for a bombing halt would be met.[46]

Nixon repeatedly opposed the "imposition by *negotiation*" of a coalition government on South Vietnam.[47] He was willing, however, to accept the possibility of a freely elected coalition government: ". . . any individual in South Vietnam who gave up the use of force and who agreed to accept the democratic process would be allowed both to vote and to participate in government."[48]

Humphrey's view was similar, though again a shade more dovish. He said (quoting Robert Kennedy) that he was opposed "to forcing a coalition government on the government of Saigon, a coalition with the Communists even before we begin the negotiations."[49] Unlike Nixon, however, he left open the possibility of negotiating a coalition: ". . . we expect that the National Liberation Front and the Viet Cong will play some role in the future political processes of South Vietnam, but that should be determined by the negotiators and particularly by those people of South Vietnam."[50] Like Nixon, he favored free elections in South Vietnam, "with all people, including members of the National Liberation Front and other dissident groups, able to participate in those elections if they were willing to abide by peaceful processes."[51] He stated explicitly, while Nixon merely implied, that he was willing to accept a coalition government if one were elected in South Vietnam.[52]

Taken as a whole, the Vietnam positions of both candidates amounted to advocacy of war as usual, with a rather gradual de-escalation of American effort if and when certain conditions were met. Members of the public were entirely justified in seeing Nixon and Humphrey as standing close together near the center of the Vietnam policy scale, far from the extremes of immediate withdrawal or escalation for complete military victory. One major explanation for the absence of Vietnam policy voting, therefore, is not that the public failed to perceive reality, but that in reality there was little difference between the candidates.

Pushing the inquiry one step further, we may ask why the two major party candidates took such similar positions on Vietnam. Plainly such a finding is consistent with the predictions of many spatial models of electoral competition.[53] But a judgment about the general validity of those theories should not rest on evidence concerning a single issue in a single election; it requires a broader data base. If study of candidates' behavior shows that major party nominees tend to take highly similar positions on all issues, then we will be able to conclude that the two party electoral system itself inhibits policy voting. If candidates tend to converge under certain conditions but not under others—if, for example, the degree of issue salience, or opinion polarization, or party cleavage on the issue affects the extent to which candidates converge —then these same factors may account for variations in the extent of issue voting.[54]

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[38] Televised address, Salt Lake City, September 30, 1968, p. 5.

[39] Salt Lake City address, p. 6.

[40] *NYT*, October 18, 1968, p. 35.

[41] UPI Conference, p. 9.

[42] UPI Conference, p. 9.

[43] Salt Lake City address, p. 5.

[44] Salt Lake City address, p. 5.

[45] *NYT*, October 2, 1968, p. 1, 27.

[46] It is possible that this difference resulted from greater knowledge Humphrey may have had about administration intentions. The bombing was stopped shortly before election day.

[47] UPI Conference, p. 7.

[48] UPI Conference, p. 7.

[49] Remarks to Liberal Party, p. 1.

[50] Remarks to Liberal Party, p. 2.

[51] Salt Lake City address, p. 6.

[52] *NYT*, July 20, 1968, p. 12.

Candidate Ambiguity and Policy Voting

The similarity between Humphrey's and Nixon's Vietnam positions can account for an absence of Vietnam voting among more than half of the voters—those who didn't see much difference between the two. But what of the large minority of voters who did perceive a difference of two or more points on the seven-point scale? Did they vote their Vietnam preferences? If so, why didn't they bring about a strong relationship between opinion and vote in the population as a whole?

Part of the answer is apparent when we look at the full distribution of perceptions. Those who saw a big difference between the candidates' positions were quite unable to agree which candidate was more of a hawk, and which more of a dove. To be sure, the majority considered Nixon more hawklike, but over one third thought that Humphrey was more of a hawk than Nixon. (See Table 2.)

Among those who thought Humphrey more hawklike than Nixon, the tendency of Table 1 was reversed: hawks voted strongly for Humphrey, and doves for Nixon. These people of course counterbalanced some of the doves who voted for Humphrey and hawks who voted for Nixon. Their effect was to decrease the relationship found in Table 1. It does not follow, however, that Americans voted their Vietnam preferences to a greater extent than Table 1 indicates. On the contrary, most people's percep-

Table 2. Perceptions of Differences Between the Vietnam Positions of the Major Party Candidates, November 1968

			Amount and Direction of Difference	
Humphrey more hawkish	17% (152)	1.0% (8)	6	Humphrey much more hawkish
		0. (2)	5	
		1. (13)	4	
		6. (52)	3	
		9. (77)	2	
Difference slight or nonexistent	57% (523)	11. (104)	1	
		29. (260)	0	No difference
		18. (159)	−1	
Nixon more hawkish	26% (236)	14. (128)	−2	
		9. (79)	−3	
		2. (18)	−4	
		0. (4)	−5	
		1. (7)	−6	Nixon much more hawkish

Entries are percentages of those voters locating both candidates on the 7-point Vietnam scale who saw a given amount of difference between the two. The amount and direction of difference in perceptions is the arithmetic difference between the scale scores given the two candidates.

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[53] Hotelling, "Stability in Competition"; Downs, *An Economic Theory of Democracy;* Davis, "Mathematical Model of the Electoral Process."

[54] Arthur Smithies, "Optimum Location in Spatial Competition," *Journal of Political Economy,* 49 (June 1941), 423–439, hypothesizes, on the basis of certain assumptions about turnout, that polarization of opinions could prevent convergence. Otto A. Davis and Melvin Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," in *Mathematical Applications in Political Science,* ed. Joseph L. Bernd (Dallas: S.M.U. Press, 1966), pp. 175–208, suggests that the need to win nomination could lead to divergent policies on party-related issues.

We are presently completing a study of the positions which presidential candidates have taken on a number of policy issues. Our preliminary findings indicate that it is possible to specify conditions under which Republican and Democratic nominees do and do not tend to take similar positions.

tions were too confused to play a part in voting decisions.[55]

Those who saw a big difference between Humphrey and Nixon—a difference in either direction—were generally perceiving each candidate as standing wherever they wanted him to stand. They projected their own opinions onto their favored candidate. Among Republicans, who mostly favored Nixon, extreme hawks thought that Nixon was an extreme hawk; extreme doves thought he was an extreme dove; and those in the middle thought that Nixon stood in the middle! The relationship between opinion and perception was quite strong. (See Table 3.) Similarly, among Democrats, extreme hawks tended to think Humphrey was an extreme hawk; extreme doves thought Humphrey an extreme dove; and those in the middle thought he stood in the middle.[56] (See Table 4.) Moreover, Republicans tended to perceive Humphrey as taking a position quite different from their own opinion.[57]

Many of those who saw a big difference between Nixon and Humphrey, in other words, were responding to their own wishes. Their perceptions were the result of intended vote, not the cause. These people were not engaged in policy voting.

This finding does not, on its face, cast great credit upon the American voter: it seems to support the view that voters are ignorant and incapable of rational political decisions. Again, however, it is not necessary to rely entirely upon an explanation based on voters' deficiencies. Further examination of the candidates' behavior shows that Nixon and Humphrey both made it quite easy to misperceive their Vietnam positions, and quite hard to vote on the basis of the slight differences which actually existed. Both candidates were very vague and ambiguous.

Both Nixon and Humphrey avoided the Vietnam issue when they could; when they did discuss it, they talked in vague generalities; and only on very rare occasions did they mention specific policy proposals. It is possible for scholars, after reading all their speeches and statements, to arrive at judgments of what their "real" positions were; but the ordinary citizen may be forgiven if he failed to penetrate the haze of vague hints which alternated with total silence about Vietnam in most of the candidates' rhetoric.

Nixon was a master of ambiguity on Vietnam. Early in the campaign he promised that new leadership "will end the war and win the peace in the Pacific. . ."[58] Again and again he declared, "The war must be ended. It must be ended honorably. . ."[59] Few Americans could disagree with that, or with his insistence that the South Vietnamese should replace American troops. But Nixon refused to explain *how* he would end the war, on the grounds that an explanation might interfere with the efforts of the Johnson administration to achieve a settlement and would weaken his own bargaining position if he became President.[60] He canceled his first major speech on Vietnam when President Johnson scheduled an address for the same day.[61] After the March 31 bombing limitation he said he would withhold criticism of Vietnam policy for two or three weeks to see how the Viet Cong and others reacted;[62] at the end of that period he said he would continue to remain silent "as long as there is hope for successful negotiation," so as to avoid undercutting Presi-

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[55] We are defining policy voting as a process in which an individual's policy preference, together with his perception of the candidates' stands on the issue, influences his vote. Under this definition it is possible for citizens with varying perceptions—even misperceptions—to engage in policy voting. (See footnote 13, above). The crucial question is whether opinions and perceptions are causally prior to the vote.

We present evidence, below, that although those who saw a big difference between Nixon and Humphrey tended to have congruent opinions, perceptions, and votes, much of the congruence resulted from projection rather than policy voting: vote intentions (as determined by party identification) were causally prior to perceptions.

[56] The greater strength of the relationship for Nixon may in part reflect his greater ambiguity on Vietnam, which made projection easier; but it also results from the fact that Republicans tended to like Nixon more than Democrats liked Humphrey. Republicans therefore felt a greater need to make their perceptions of Nixon's position congruent with their own opinions.

[57] Negative projection of the opposing candidate's position was more marked among Republicans than Democrats. Only 5 per cent of these Republicans saw Humphrey standing at exactly the same point as themselves, compared to the 14 per cent which would have been expected on the basis of the marginals. The overall negative relationship was significant at better than $p = .001$, by a chi-square test. The corresponding relationship for Democrats' perceptions of Nixon was not significant at the $p = .10$ level, however. The reason for this difference appears to be that Republicans felt negative toward Humphrey, but Democrats were merely neutral about Nixon and had less reason to project his position away.

All these tables are based only on voters who saw a large difference between the candidates. Projection was also widespread among other groups, such as nonvoters, but it was much less common among voters who saw little difference between Nixon and Humphrey. We are grateful to Sue Bessmer for assistance in the analysis of perceptions.

[58] *NYT*, March 5, 1968, p. 1.

[59] "Vietnam," August 1, 1968.

[60] *NYT*, March 11, 1968, p. 1.

[61] *NYT*, March 31, 1968, p. 39.

[62] *NYT*, April 2, 1968, p. 1.

Table 3. Projection of Nixon's Vietnam Position by Republicans Who Saw a Substantial Difference Between Nixon and Humphrey*

Perception of Nixon's Stand	Policy Preference on Vietnam Immediate Withdrawal				Complete Military Victory	
Immediate Withdrawal	20% (4)	0 (0)	0 (0)	0 (0)	0 (0)	3 (4)
	55 (11)	42 (10)	19 (5)	4 (1)	14 (3)	25 (30)
	20 (4)	25 (6)	54 (14)	7 (2)	9 (2)	23 (28)
	0 (0)	33 (8)	23 (6)	89 (25)	55 (12)	43 (51)
Complete Military Victory	5 (1)	0 (0)	4 (1)	0 (0)	23 (5)	6 (7)
	17 (20)	20 (24)	22 (26)	23 (28)	18 (22)	100 (120)

Note: Tau b = .50 Correlation ratio (eta) = .376.

* This table is based on Republicans who voted and who perceived a difference of more than one point on the Vietnam scale between Nixon's position and Humphrey's. Scores of "2" and "3" and of "5" and "6" on the seven-point scale have been combined.

dent Johnson.[63] He claimed that words from an irresponsible candidate might "torpedo" the deliberations.[64] Throughout the autumn campaign, with few exceptions,[65] Nixon confined himself to generalities and condemned loose talk which might weaken our diplomatic hand.[66]

Nixon's typical approach to Vietnam is illustrated by his acceptance speech, "I See a Day . . . ," which was very carefully composed and which set the tone for this autumn campaign.[67] Of the 253 sentences in the speech, only 25, or about 10 per cent, were devoted to Vietnam.[68] Of those 25 sentences, at most six, or 2 per cent of the entire speech, concerned future actions and policies.[69] The most specific promises were two: that he would "begin with Vietnam," in making a "complete reappraisal of America's policies in every section of the world"; and that "the first priority foreign policy objective of our next Administration will be to bring an honorable end to the war in Vietnam."[70] The voter could not hope to find much information here—or in the TV spots or stump speeches which echoed the acceptance speech—about what Nixon proposed to do in Vietnam.

Rather than dealing with the specifics of future Vietnam policy, Nixon's rhetoric heavily emphasized the failure of past policies. In the acceptance speech, of his 25 Vietnam-related sentences, 13—about half—concerned the past. He evoked the image of "Americans dying on distant battlefields abroad."[71] He asserted that "[w]hen the strongest nation in the world can be tied down for four years in a war in Vietnam with no end in sight . . . it's time for new leadership. . ."[72] He complained that for four years the Johnson administration had had at its disposal the greatest military and economic advantage one nation had ever had over another in a war; that America's fighting men had set an unsurpassed record for courage and sacrifice; that the "Loyal Opposition" had given full support. But "[n]ever has so much military and economic and diplomatic power been used so ineffectively. And if after all of this time and all of this sacrifice and all of this support there is still no end in sight, then I say the time has come for the American people to turn to new leadership—not tied to the mistakes and the policies of the past."[73] Condemnation of past policies on Vietnam was safe, and had great appeal for most Americans. Moreover, attacks on the past, without specifics about the future, left voters free to imagine that Nixon stood for whatever they wanted him to stand for.

Humphrey, in the autumn of 1968, was a little more specific about Vietnam policy than Nixon was. At least he delivered one major

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[63] *NYT*, April 20, 1968, p. 17.

[64] *NYT*, May 4, 1968, p. 18.

[65] Most notably at the UPI Conference on October 7, where, in effect, he responded to Humphrey's Salt Lake City speech.

[66] Statement October 25, 1968, p. 2.

[67] "I See a Day . . . ," *Nixon Speaks Out* (New York: Nixon-Agnew Campaign Committee, 1968), pp. 277–289.

[68] This count is a generous one. It includes such allusions to Vietnam as, "We see Americans dying on distant battlefields abroad," p. 278; and such Vietnam-related matters of "prevent[ing] more Vietnams," p. 281, by asking "other nations in the Free World to bear their fair share of the burden of defending peace and freedom around this world," p. 281, 282.

[69] Again, the count is generous; it includes sentences disavowing any promise to "eliminate all danger of war," p. 280; promising "action—a new policy for peace abroad," p. 280; declaring the policy of preventing "more Vietnams," p. 281; and announcing that the time has come for other nations to bear more burdens of defending peace and freedom, p. 282.

[70] "I See a Day . . . ," p. 281.

[71] "I See a Day . . . ," p. 278.

[72] "I See a Day . . . ," p. 280.

[73] "I See a Day . . . ," p. 281.

Table 4. Projection of Humphrey's Vietnam Position by *Democrats Who Saw a Substantial Difference* Between Nixon and Humphrey*

	Policy Preference on Vietnam					
Perception of Humphrey's Stand	Immediate Withdrawal				Complete Military Victory	
Immediate Withdrawal	28% (11)	17 (4)	2 (1)	4 (1)	2 (1)	9 (18)
	23 (9)	46 (11)	20 (12)	36 (10)	12 (5)	24 (47)
	21 (8)	21 (5)	53 (32)	18 (5)	17 (7)	30 (57)
	18 (7)	13 (3)	18 (11)	36 (10)	19 (8)	20 (39)
Complete Military Victory	10 (4)	4 (1)	7 (4)	7 (2)	50 (21)	17 (32)
	20 (39)	12 (24)	31 (60)	15 (28)	22 (42)	100 (193)

Note: Tau b = .33 Correlation ratio (eta) = .211.

* This table is based on Democrats who voted and who perceived a difference of more than 1 point on the Vietnam scale between Nixon's position and Humphrey's. For clarity in presentation, scores of "2" and "3" and scores of "5" and "6" have been combined. This collapsing does not materially affect the form or strength of the relationship.

speech concerning Vietnam on national television,[74] and quoted it from time to time. Yet even that speech was no model of clarity. As we have seen, he declared he favored a bombing halt, but then made it contingent on concessions by North Vietnam; he favored reducing the number of U.S. troops in Vietnam, but did not indicate when withdrawals would start, or how fast they would proceed, or whether all troops would be removed. He was also unclear about whether he would negotiate a coalition government.

Those who missed the Salt Lake City speech, or forgot it, had even less to go on. Like Nixon, Humphrey observed silence on Vietnam during much of the year. Often he simply ignored the issue, even in major foreign policy speeches.[75] Sometimes he explicitly argued that it should not be made an issue, so as not to upset the negotiations.[76]

Humphrey's typical treatment of Vietnam, when he mentioned it, was as vague as Nixon's. Of 182 sentences in his acceptance speech, "A New Day for America," only 14, or 8 percent, dealt with Vietnam.[77] Of those, only three, or less than 2 per cent of the speech, said anything about future policy in Vietnam. The first simply alluded to "the necessity for peace in Vietnam. . ."[78] The second, and most specific, pledged he would "do everything within my power to aid the negotiations and to bring a prompt end to this war."[79] In the third, he promised to apply to the search for peace in Vietnam the lesson that "the policies of tomorrow need *not* be limited by the policies of yesterday."[80] Humphrey, unlike Nixon, did not dwell upon the past; the bulk of his comments on Vietnam, in fact, amounted to an exhortation to forget the past and consider what to do next—but without ever suggesting what *he* advocated doing next.

Where the past presented an opportunity to Nixon, it posed a painful dilemma for Humphrey. As a member of the administration he was identified with failure in Vietnam; he could not hope to persuade the voters that past policies had been successful, yet he could not, without gravely offending the President (and apparently his own conscience), make a clean break with the past. Accordingly, before he arrived at a satisfactory compromise in the Salt Lake City Speech, he alternated between protestations of loyalty to current policy, and hints that he really disagreed with it.

When Johnson withdrew from the race and Humphrey became a candidate, he remained Vice President, and he often continued to define his position in terms of what "we" (the administration) think.[81] Sometimes, however, he seemed to stray from the fold, as when he asserted that it had been agreed that the N.L.F. could sit on the North Vietnamese side in the Paris negotiations.[82] There were recurrent rumors of his private disagreements with L.B.J., which Humphrey or his advisors denied.[83] He declared that a Vice President should be loyal:

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[74] This was the Salt Lake City speech referred to above.

[75] *NYT*, April 23, 1968, p. 1; June 5, 1968, p. 34.

[76] *NYT*, May 27, 1968, p. 28; July 8, 1968, p. 31; October 17, 1968, p. 39; October 27, 1968.

[77] "A New Day for America," Chicago, August 29, 1968.

[78] "A New Day for America," p. 4.

[79] "A New Day for America," p. 5.

[80] "A New Day for America," p. 5.

[81] See, for example, the interview in *NYT*, June 23, 1968, p. 58.

[82] *NYT*, May 18, 1968, p. 1.

[83] *NYT*, June 19, 1968.

"We don't need an Aaron Burr in this Republic."[84] He tried to compromise with McCarthy forces on the Vietnam plank of the Democratic platform, but then rejected compromise, apparently as a result of pressure and maneuvering by President Johnson.[85] After his nomination, Humphrey vacillated between claiming that he would be his own man on Vietnam, and asserting that there was no great cleavage with the administration.[86] On one occasion he evoked presidential displeasure by speculating about imminent troop withdrawals, and had to make a humiliating series of clarifications and retractions.[87]

With these contradictory hints, and his relative silence about specific policy, Humphrey—like Nixon—made it hard for voters to perceive his real position. It would be an error to excuse too much: those who thought Humphrey an extreme hawk, or Nixon an extreme dove, ought to have had enough evidence to come to a different conclusion. Yet there can be no doubt that the voters were responding to ambiguous stimuli. The candidates' behavior encouraged projection and, by obscuring what differences actually existed between them, inhibited policy voting.

Again we must push the inquiry one step further, and ask why Nixon and Humphrey were both so ambiguous. The case of Vietnam is consistent with theories which suggest that ambiguity may be the rule in candidates' rhetoric. Vagueness defuses issues. If a candidate is vague, he fails to gain new support which might come from those who would agree with his position, if he took one; but he also avoids alienating the many who would not agree with his position, whatever it was. In many cases, or even all cases, ambiguity might profit a candidate more than any single specific position could.[88]

Still, evidence from the study of a single issue does not permit generalizations about the electoral process. The question of candidates' ambiguity, like that of convergent issue positions, must be answered in the context of a cross-section of issues. If it is the case that candidates are always ambiguous, that would constitute a way in which candidates' behavior and the electoral process itself regularly inhibited issue voting.[89]

The Ability to Vote Policy Preferences

We have shown that the candidates' behavior was such that it could explain the general absence of Vietnam policy voting in 1968, without recourse to arguments about the ignorance or incapacity of voters. Yet we have not refuted the ordinary explanation for low issue voting, since we have not yet shown that, under more favorable circumstances, citizens can or do perceive objective differences between candidates' policy stands, and vote on the basis of those differences.

In 1968, several candidates other than Nixon and Humphrey took somewhat clearer positions on Vietnam, and stood somewhat farther apart from each other on the issue. Examination of the public's perceptions of those candidates' issue positions, and of public evaluations of those candidates, permits us to put citizens' capacities for issue voting to a fairer test. The evidence shows that Americans *were able,* to a substantial extent, to vote their policy preferences.

Eugene McCarthy, in challenging President Johnson for the Democratic nomination, campaigned heavily on the Vietnam issue. McCarthy talked much about the past—the morality of the war, misrepresentations by the administration, usurpations of Senatorial power, and the like; his proposals for future policy were surprisingly few, and not free from ambiguity.[90] Still, he said enough to make clear that he favored de-escalation of the war.

Early in the year, McCarthy urged that the bombing of North Vietnam be stopped, in order to bring about negotiations.[91] He also criticized the use of napalm and antipersonnel bombs on the Vietnamese.[92] After the March 31 bombing limitations, he ceased to emphasize the matter of bombing, and even said it had become part of general tactics; he would leave it to the military to decide if it was necessary to bomb in order to prevent supply and infiltration.[93] In August, however, he proposed that the bombing of North Vietnam, as well as naval and artillery bombardment, be stopped immediately.[94]

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[84] *NYT,* July 19, 1968, p. 18.

[85] *NYT,* July 20, 1968, p. 12; August 20, 1968; p. 1; August 27, 1968, p. 1; August 28, 1968, p. 30.

[86] *NYT,* September 9, 1968, p. 46.

[87] *NYT,* September 10, 1968, p. 32; September 11, 1968, p. 1, 28; September 12, 1968, p. 36.

[88] Similar reasoning is given in Downs, *An Economic Theory of Democracy,* pp. 135–137. Candidate ambiguity is predicted under special circumstances in Shepsle, "The Strategy of Ambiguity."

[89] Stanley Kelley, *Political Campaigning,* pp. 50–84, found that Eisenhower and Stevenson were vague concerning practically all issues in 1956. The televised debates of 1960 compelled Nixon and Kennedy to be somewhat more specific. Stanley Kelley, Jr., "The Presidential Campaign," in Paul T. David, ed., *The Presidential Election and Transition 1960–61* (Washington, D.C.: Brookings, 1961), p. 87.

We have found that Humphrey and Nixon took ambiguous positions on most policies in 1968. Nixon, especially, was quite ingenious at appearing to say much while actually saying little.

[90] A complete set of McCarthy's speeches was generously made available to us by Robert Axelrod and John D. Shilling. Sally Soth has helped greatly in analyzing these speeches.

[91] Television interview on "Issues and Answers," January 7, 1968, p. 14. This and subsequent page numbers refer—unless otherwise noted—to official press releases and transcripts.

McCarthy, like all other candidates, disavowed any desire for "immediate" or "unilateral" withdrawal of U.S. troops from Vietnam.[95] He did, however, endorse a pull-back of American troops to enclaves, rather than trying to hold all of South Vietnam;[96] he urged that "search and destroy" missions be cut back.[97] If the South Vietnamese were unwilling to negotiate a settlement with the N.L.F., he suggested that the U.S. should carry out some "phased withdrawals" of troops until they did so.[98] In August, he advocated a cease-fire and complete withdrawal after an interim coalition government was agreed to.[99]

Coalition government was the key element in McCarthy's position on Vietnam. Repeatedly he advocated a coalition government and said that if the present South Vietnamese government did not agree, they ought to be told to carry on the war by themselves.[100] He stressed that since we had failed to destroy the N.L.F. on the field of battle, we must be prepared to allow it to be a force in the future government of South Vietnam.[101] He warned that the U.S. must accept the possibility of an eventual reunification of North and South Vietnam.[102]

McCarthy's most definitive—and best publicized—proposals about Vietnam came in late August, with the controversy over the Democratic platform. He advocated a two-stage plan for settlement. First, the U.S. would stop bombing North Vietnam, reduce the level of conflict, and negotiate a new interim government for South Vietnam with substantial participation by the N.L.F. Second, the coalition government would prepare for elections with international supervision and guarantees. Before the election, the U.S. would withdraw its troops and a cease-fire would be maintained. The new, freely elected government would then deal with North Vietnam as it chose.[103]

Thus, McCarthy was a fairly clear—though not extreme—dove. He put forth a program which involved de-escalation of the fighting and a conciliatory settlement. It is not so easy, however, to find a hawkish candidate. After the fortunes of war appeared to turn against the U.S. with the Tet offensive of early 1968, no presidential candidate very forcefully advocated military escalation.

Ronald Reagan, for example, had hawkish inclinations. He urged in early 1968 that the U.S. resist any communist proposals for a coalition government.[104] He favored escalation: the U.S. should stop the entrance of supplies to Haiphong harbor, and should invade North Vietnam or pose the threat of doing so.[105] In May, he suggested that if the Communists stalled in the Paris Peace talks, the U.S. should serve notice and then "kick the devil out of them."[106] If it proved desirable to invade North Vietnam, South Vietnamese troops should be used and should be supported logistically by the U.S.[107] At hearings on the Republican platform, Reagan urged that the U.S. stand firm and fight to win.[108]

Reagan's comments about Vietnam were infrequent, however, and received very little national publicity.[109] Until the eve of the convention Reagan disavowed presidential ambitions and did not enter any primaries or conduct a national campaign. To most citizens, he was just another governor who was occasionally mentioned as a dark-horse candidate. It would not be a fair test of public abilities, therefore, to ask that citizens perceive his inconspicuously hawkish Vietnam position, or that they judge him on the basis of Vietnam.

The nearest thing to a hawk among active presidential candidates in 1968 was George Wallace. Even Wallace was not an absolutely clear case, since Vietnam concerned him much less than civil rights and urban disorders, and he said relatively little about it.[110] When Wallace did discuss Vietnam, he emphasized peripheral issues: he wanted to "crack down" on people who asked for a communist victory in Vietnam, and on allies who traded with North Vietnam or otherwise failed to support the U.S. effort.[111] In his standard speech he declared that if he became President:

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[92] Remarks, Stanford University, January 15, 1968, p. 4.

[93] Televised news confeernce, June 2, 1968, p. 6.

[94] Vietnam platform proposal, *NYT*, August 18, 1968, p. 66.

[95] Press conference, Indiana University, April 18, 1968, p. 4; televised interview on "Meet the Press," July 7, 1968, p. 2.

[96] Press conference, Washington, D.C., June 12, 1968, p. 7.

[97] Remarks, San Francisco, May 29, 1968, p. 5.

[98] Response to question, Stanford University, January 15, 1968, pp. 3–4.

[99] Vietnam platform proposal, p. 66.

[100] Response to question, Wisconsin State University at Eau Claire, February 21, 1968, p. 12; press conference, Indiana University, April 18, 1968, p. 3.

[101] Remarks, San Francisco, May 29, 1968, p. 5.

[102] Response to question, University of Oregon, May 24, 1968.

[103] Vietnam platform proposal, p. 66.

[104] *NYT*, January 24, 1968, p. 55.

[105] *NYT*, March 4, 1968, p. 16.

[106] *NYT*, May 24, 1968, p. 20.

[107] *NYT*, May 27, 1968, p. 30.

[108] *NYT*, August 1, 1968, p. 20.

[109] In the seven months from January to the Republican convention, for example, the *New York Times* reported Reagan's comments on Vietnam only nine times. All of these reports were very brief and none were prominently featured.

> I'm going to ask my Attorney General to seek an indictment against every professor in this country who calls for a communist victory (voice rising) and see if I can't put them under a good jail somewhere. (loud cheers). I'm sick and tired of seeing these few college students raise money, blood, and clothes for the Communists and fly the Vietcong flag; they ought to be dragged by the hair of their heads and stuck under a good jail also.[112]

Wallace's tone was certainly belligerent, but his specific policy proposals were only mildly hawkish. In his standard speech he expressed hope for success of the Paris talks; if they failed he would ask the Joints Chiefs of Staff for a plan to attain military victory, with conventional weapons only.[113] In September Wallace declared he was "not advocating invading anybody at the moment," nor did he favor using nuclear weapons.[114] He opposed a declaration of war and denied that any large number of troops would be needed to win.[115] He did not even totally oppose a bomb halt, but insisted only that North Vietnam be required to make concessions: stop attacks on South Vietnamese cities, de-escalate in the South, and stop infiltration of South Vietnam.[116] Wallace was not an extreme hawk unless perhaps by inference from his aggressive rhetoric about war protesters, and from his choice of General Curtis LeMay, the tough and outspoken former head of the Strategic Air Command, as his vice-presidential running mate.[117]

Despite the qualifications we have made about their tendencies to be moderate and ambiguous, McCarthy and Wallace represented much more clear and sharply different Vietnam policy options than Nixon and Humphrey did. How did the public react to these stimuli? How clearly did they perceive what Wallace's and McCarthy's positions were, and to what extent did they take Vietnam into account in evaluating the candidates?

Public perceptions of Wallace's and McCarthy's Vietnam positions were fairly accurate. In August, people saw the two as standing at far different points on the seven-point scale: McCarthy near the dovish end, between points two and three, and Wallace near the hawkish end, between points five and six. (See Figure 2.) Such ratings would seem to be faithful reflections of the positions which the candidates had actually taken in their speeches. A panel of well-informed judges, who rated the candidates' positions at approximately the same time the public did, came to quite similar conclusions.[118]

We should not understate the extent to which perceptions of the two candidates were scattered across the scale. A few people, in fact, misperceived Wallace's or McCarthy's position so drastically that they placed one of them at the extreme point on the wrong end of the scale.[119] These individuals were distinctly in the minority, however. In mid-August, before McCarthy had made his clearest Vietnam statements, 59 per cent were able to place him at one of the three positions on the dovish side of the scale; only 21 per cent saw him on the hawkish side of the midpoint. Similarly, 61 per cent saw Wallace as a hawk, and only 22 per cent thought him a dove. In November, after Wallace had received more media exposure and had picked LeMay as his running mate, perceptions were still more clear: 75 per cent saw him as a hawk, and only 16 per cent as a dove.[120]

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[110] Wallace, even more than other candidates, relied upon a single basic speech, which he modified only slightly as the campaign progressed. We have transcribed a tape recording distributed by the American Independent Party of a typical speech which Wallace delivered in Charlotte, North Carolina, in mid-June, 1968. We have relied upon the *New York Times* for reports of deviations from the basic speech.

[111] *NYT*, July 5, 1968, p. 14; July 27, 1968, p. 10; July 30, 1968, p. 27.

[112] Remarks, Charlotte, North Carolina, June 1968.

[113] Remarks, Charlotte, N.C.

[114] *NYT*, September 2, 1968, p. 17.

[115] *NYT*, September 26, 1968, p. 53.

[116] *NYT*, October 21, 1968, p. 38.

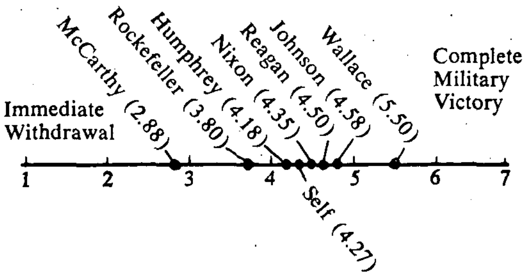
[117] The choice of LeMay was very likely a mistake, which undercut Wallace's careful effort to be acceptable to those who opposed the war. LeMay disclaimed any intention of using nuclear weapons in Vietnam, *NYT*, October 4, 1968, pp. 50 ff.; but his hawkish image persisted.

[118] The judges were 22 journalists, international relations specialists, and economists, from a variety of geographical areas. They gave McCarthy a median rating of approximately 2.0, and Wallace a rating of 6.6. They located Humphrey and Nixon much closer together and nearer the middle of the scale, at 4.0 and 5.0, respectively.

[119] In August, 4 per cent rated McCarthy as an extreme hawk, and 6 per cent rated Wallace as an extreme dove. The percentages are based on those respondents, some 70 per cent of the sample, who made a rating.

[120] Slightly more than half the respondents placed him at the most extremely hawkish position on the scale.

Figure 2. Average Perceptions of Candidates' Vietnam Positions, August 1968



Entries are the median ratings of respondents' own opinions and their perceptions of candidates' positions on the seven-point Vietnam scale.

We cannot ascertain how much difference individuals saw between Wallace's and McCarthy's positions in November, as we did for Nixon and Humphrey, because we lack November data on McCarthy.[121] Even in mid-August, however, before the public had received the full input of information about either candidate's position, four times as many people thought Wallace more hawkish than McCarthy as thought the opposite.[122]

We can conclude, then, that the public displayed a reasonable ability to perceive candidates' issue positions, when those positions were fairly clear. Next we must inquire whether people took those perceptions and their own Vietnam preferences into account when they evaluated the candidates.

In order to answer this question, we conducted what might be called a "mock election" between McCarthy and Wallace. Using November data on respondents' evaluations of each candidate,[123] we calculated which of the two each respondent favored more. Such comparative evaluations can be taken as closely resembling votes.[124] We cross-tabulated respondents' hypothetical votes for McCarthy or Wallace by their Vietnam policy preferences, just as we had done with actual votes for Humphrey and Nixon in Table 1.

The results are unmistakable: in evaluating McCarthy and Wallace, Americans took their Vietnam preferences strongly into account. McCarthy was more popular than Wallace among holders of all shades of opinion, but his share of the "vote" varied greatly: doves supported McCarthy at a far higher rate than hawks did.[125] (See Table 5.) Vietnam opinions, in fact, correlated with comparative evaluations at the high level of .29; they accounted for more than 8 per cent of the variance, many times as much as in the case of Humphrey and Nixon.

Considering the other factors which were relevant to choices between McCarthy and Wallace—their personal styles and abilities, for example, and their positions on civil rights—the impact of Vietnam preferences was very great indeed.

To be sure, the idea of a presidential election between McCarthy and Wallace is somewhat fanciful, since Wallace did not seek a major party nomination. Our point does not really require any speculation about the outcome of a hypothetical vote; it is made sufficiently well by the outcome of actual comparative evaluations.

The same point can be made, if it seems more convincing, with a pair of candidates who might conceivably have been nominated in 1968: McCarthy and Reagan. As we have indicated, Reagan's Vietnam position had low visibility, and perceptions of it were blurred. Yet in a mock election between McCarthy and Reagan, Vietnam preferences had a sizable impact—much greater than in the actual Humphrey-Nixon election.[126] We can conclude that, when the American people are presented with a clear choice, they are able and willing to bring their policy preferences to bear on it.

Conclusion

Despite the unusually high salience of the

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[121] In both the SRC and ORC/IEP November surveys, respondents were asked to rate the positions only of the active candidates and President Johnson.

[122] The percentages, of those who rated both candidates, were 47 per cent and 11 per cent, respectively. Forty-two per cent saw no difference between the two, however, and in addition a large number of respondents failed to assign one or both candidates to any position on the scale.

As we have seen, public perceptions of Wallace's position were clearer in November. Indirect evidence suggests that perceptions of McCarthy improved as well, after the Democratic convention.

[123] These evaluations were measured on the 100-point SRC thermometer. See Weisberg and Rusk, "Dimensions of Candidate Evaluation."

[124] Actual votes for Humphrey and Nixon correspond almost perfectly with comparative evaluations of the candidates. Brody et al., "Vietnam, the Urban Crisis and the 1968 Presidential Election," pp. 7–8.

[125] The slight nonlinearity in Table 5 suggests that a few respondents who placed themselves at the "immediate withdrawal" end of the scale may actually have favored a policy of "win or get out," and supported Wallace.

[126] Vietnam preferences can account for approximately 4 per cent of the variance. The resemblance between the McCarthy-Reagan mock election and a real election is still not perfect, of course. If McCarthy and Reagan had been nominated, party identification might have affected evaluations of them more strongly, and might have blunted the impact of Vietnam opinions somewhat. On the other hand, nomination would have increased public exposure to the candidates' positions on Vietnam, and might on balance have increased the effect of Vietnam preferences.

Table 5. Vietnam Policy Preferences and Comparative Evaluations of McCarthy and Wallace, November 1968

	Opinion on Vietnam		
Preferred Candidate	Pull Out Entirely	Keep Soldiers, Try to End Fighting	Take Stronger Stand, Even if it Means Invading North Vietnam
Wallace	15%	13	40
	(23)	(42)	(125)
McCarthy	85%	87	60
	(135)	(292)	(187)
	100	100	100
	(158)	(334)	(312)

Note: Tau b = .26 Correlation ratio (eta) = .095.
Pearson correlation (r) = .26 $r^2 = .068$.

	Opinion on Vietnam						
Preferred Candidate	Immediate Withdrawal 1	2	3	4	5	6	Complete Military Victory 7
Wallace	15%	11	4	15	27	45	45
	(16)	(8)	(3)	(39)	(26)	(38)	(68)
McCarthy	85	89	96	85	73	55	55
	(90)	(68)	(66)	(222)	(71)	(46)	(82)
	100	100	100	100	100	100	100
	(106)	(76)	(69)	(261)	(97)	(84)	(150)

Note: Tau b = .27 Correlation ratio (eta) = .117.
Pearson correlation (r) = .29 $r^2 = .084$.

The preferred candidate is the one given a higher rating on the 100 point "thermometer" of candidate evaluations. Those who rated the two candidates equally are omitted.

In order to ensure comparability with Table 1, only those who voted are included in the table.

Vietnam war in 1968, the conventional wisdom about American electoral politics remained true; policy preferences had little effect on the major-party vote. The reason for this slight impact of a great issue lay in voters' perceptions of where the candidates stood on Vietnam. Most people saw little or no difference between Nixon's position and Humphrey's. Further, those who saw a difference did not have firmly grounded perceptions, but tended to imagine the candidates as standing wherever they wanted them to stand.

The widespread failure to see any difference between the candidates, and general confusion over where they stood, did not result from inability on the part of the public to perceive correctly. Most people were able to perceive that George Wallace favored a hard line in Vietnam and that Eugene McCarthy wanted to de-escalate the war. Indeed, people's comparative evaluations of Wallace and McCarthy were very strongly influenced by their preferences about Vietnam. Americans seem to have had the capacity to vote their policy preferences.

In fact, those who saw little difference between Nixon and Humphrey on Vietnam were responding to reality. There was only a slight difference between them, as evidenced by their campaign speeches. Those who misperceived the candidates' positions were given ample opportunity by the candidates' ambiguity. Nixon and Humphrey rarely discussed specific Vietnam policies, and avoided the issue of Vietnam altogether when they could.

The similarity of the Vietnam positions of the major party candidates was consistent with

—though it could not in itself confirm—theories of electoral competition which predict that vote-maximizing candidates in a two-party system adopt similar issue positions. The ambiguity of Humphrey and Nixon was likewise consistent with theories which suggest that ambiguity is frequently advantageous.

In this case, at least, the behavior of candidates seems to have inhibited the occurrence of policy voting. If similar candidates and ambiguous rhetoric are the rule in American elections, they may well have depressed voting on other issues, and in other elections as well. Our picture of the ignorant voter may be, as V.O. Key, Jr. has suggested, largely a reflection of the choices which he is offered.[127] Even if policy voting is regularly inhibited by the electoral process, however, the implications of this finding for democratic theory are not straightforward. Those who rejoice in the opportunity to vote for a candidate they like, regardless of the outcome of the election, may well regret excessive similarity between major party candidates. Those who seek a democratic outcome from elections, however, may reply that similarity of candidates is not at all to be disdained, so long as both of them stand for the policies most preferred by the public. The proponents of convergence could point out that, as is indicated in our Figure 1, the American public (for better or worse) got about the Vietnam positions it wanted from both candidates in 1968. Whichever one had been elected would, if he had kept his campaign promises, have pursued a Vietnam policy quite close to the midpoint of public opinion.[128]

If the implications of candidate similarity are controversial, the consequences of ambiguity are rather obvious. Vague candidates cannot contribute much to the education of the public, and cannot facilitate the casting of policy-oriented votes when there *is* a real difference between candidates. Murkiness in political rhetoric is almost universally deplored. But if candidates have a real incentive to be vague, and ambiguity is rooted in the electoral process itself, it may be quite resistant to reform.

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[127] V. O. Key, Jr., *The Responsible Electorate,* pp. 2, 7.

[128] When we consider a single issue, upon which preferences are jointly single-peaked, a policy at the median of public opinion is the only policy which would not be defeated by any other in a series of two-way votes. The median may therefore be said to represent what "the public wants."

As Figure 1 shows, the public generally perceived both Nixon and Humphrey as standing very close to the median of public opinion on the seven-point Vietnam scale. Commercial poll data and the candidates' speeches confirm that both candidates stood close to the median of public opinion on specific aspects of Vietnam policy.

Freshman Committee Assignments and Re-election in the United States House of Representatives*

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Many factors have been cited as important in assigning freshman congressmen to committees. Nicholas Masters reports that the member's background, the influence and activity of one's dean, interest group support, a reputation for responsibility, along with considerations of geographic, ethnic, religious and factional balance are weighed before making assignments.[1] Charles Clapp adds to these variables preferences of committee chairman, the aspirant's age, and his positions on major issues likely to come before the committee.[2]

Both observers agree, however, that one factor is paramount. In the final analysis assignments are made to facilitate the recipients' re-election. Clapp writes:

> Where competition for vacancies exists, choice assignments are often given to members representing "marginal" congressional districts as opposed to those from districts firmly held by the party.[3]

Masters is even more specific, stating that:

> Although a number of factors enter into committee assignments . . . the most important single consideration . . . is to provide each member with an assignment that will help to insure his re-election.[4]

Congressmen from safe districts also judge that the good assignments are reserved for the party's needy. One legislator complains that "What few decent openings were available were given to those elected from marginal districts."[5] He is echoed by a fellow congressman, "They [the committee on committees][6] are too often inclined to give a good seat to a man from a swing district who, despite his good points, may be defeated."[7]

If committee assignments can improve the recipients' re-election chances, the group of congressmen most in need of such assistance would be freshmen from marginal districts. Therefore, this paper will first test the proposition that freshmen from marginal districts receive better initial assignments than do men from safe districts. Having done this, the electoral consequences of receiving a "good" assignment will be evaluated.

Identifying the "Good" Assignments

The rationale underlying the assumption that the narrowly elected are favored for good assignments is straightforward. It is anticipated that because of the significance of committee activities to the operation of the House, committee assignments are useful in reputation building and in developing a following among one's constituents.

> I think it's true of the Republicans as well as the Democrats . . . we want the members from our side re-elected. Committee assignments have an important bearing on this problem, because how you function in Congress, the effectiveness of your work here, depends in no small measure on the committee assignments that you are given.[8]

A committee post may enable a congressman to expedite legislation beneficial to his district, to protect local interests, or to provide some form of constituency service. To the extent that the novice can use his committee to advance district interests or to get his name before the public, he may be able to improve his chances of re-election.

Solidification of electoral position as a result of committee work may be a widely shared expectation among congressmen despite evidence that the vast majority of Americans are unlikely to be aware of their legislator's committee activities. Although scholars now generally agree that little of what happens in the political

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* Professors Richard Fenno, Roger Hanson, David Olson, and Harrell Rodgers provided most helpful critiques of earlier drafts of this paper. My assistant Jack Ethredge toiled many hours helping collect the data.

[1] Nicholas A. Masters, "Committee Assignments in the House of Representatives," *American Political Science Review,* 55 (June, 1961), 345–357.

[2] Charles Clapp, *The Congressman: His Work As He Sees It* (Garden City, New York: Anchor Books, 1964), pp. 207–240.

[3] Clapp, p. 234.

[4] Masters, p. 357.

[5] Clapp, p. 234.

[6] In this paper "committees on committees" refers to the two party committees responsible for filling vacancies on standing committees. For a description of the differences in composition and operation of the Republican and the Democratic committees responsible for filling vacancies on standing committees, see Masters.

[7] Clapp, p. 234.

[8] Donald G. Tacheron and Morris K. Udall, *The Job of the Congressman* (Indianapolis: Bobbs-Merrill, 1966), p. 152.

arena catches the attention of the masses,[9] many successful politicians do not concur. Of the congressmen interviewed, more than 80 per cent who overcame re-election challenges in 1958 credited their legislative records as a significant factor in their victories.[10] In John Kingdon's study, marginal winners by a 2:1 ratio perceived their success to be produced by stands on issues.[11]

Of course some congressmen have probably reached conclusions similar to those of political scientists concerning voter attention and knowledge. This sophistication may come only with experience so that most freshmen will perceive issues and performance as determinative of electoral fortunes. But even legislators who acknowledge that their activities go largely unnoticed by the masses may feel that some of their attentive constituents—whose vocal or financial support may be decisive—respond on the basis of committee assignments and activities.[12]

Before evaluating whether marginal freshmen receive special treatment when committee assignments are made, the conditions under which an appointment is likely to help a representative win a second term must be designated. With committees, as with women, beauty is often in the eye of the beholder; therefore we must ask, "For whom is a particular assignment a good one?"[13] The most reliable information on the differential evaluation of committees, the committee request books for each party, is infrequently accessible.[14] Nor was extensive interviewing of congressmen an available alternative for the researcher. Consequently inferences had to be drawn about what appears to be a desirable appointment for a particular congressman.

In requesting a committee, a congressman may be guided by the opportunities offered for serving certain interests of his district. The prestige of the committee, its power, and an interest in the policy responsibilities of the committee may also attract applicants.[15] In this paper attention will be confined to conditions under which assignments seem to have been made consonant with the notion of re-election promotion, that is, where there is an opportunity for serving one's constituency or building one's reputation. For five committees, available constituency characteristics indicate that a definable subset of congressmen will perceive service on a particular committee as helpful in promoting the interests of a sizable proportion of their constituents. There are six other committees (Appropriations, Armed Services, Foreign Affairs, Rules, Ways & Means, and Public Works) which it is assumed provide a basis for constituency service or reputation building for almost all congressmen.

For each committee or group of committees the freshmen thought to covet an appointment will be referred to as "interesteds."[16] If a significantly larger proportion of narrowly elected interesteds than safe interesteds is assigned to the committee, we have support for the Masters-Clapp re-election promotion hypothesis, that is, that assignments are made to improve re-election chances. In making the comparisons, the narrowly elected may be thought of as the test group; those without electoral problems constitute the control group. Throughout this paper freshmen who won less than 55 per cent of the two-party vote are classified as narrowly elected.[17]

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[9] This finding has been reached repeatedly. No attempt will be made to cite the numerous possible sources. See Angus Campbell, Philip E. Converse, Warren E. Miller, Donald E. Stokes, *The American Voter: An Abridgment* (New York: John Wiley and Sons, 1964), pp. 99–102; Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (March, 1963), 54; Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly*, 26 (Winter, 1962), 531–546.

[10] Miller and Stokes, p. 54.

[11] John W. Kingdon, *Candidates for Office: Beliefs and Strategies* (New York: Random House, 1966), pp. 25–34.

[12] The notion of "attentive constituents" is taken from a discussion of the characteristics, political knowledge, and political activities of a set of Iowans cited as political confidants by members of the Iowa legislature; see G. R. Boynton, Samuel C. Patterson, and Ronald D. Hedlund, "The Missing Links in Legislative Politics: Attentive Constituents," *Journal of Politics*, 31 (August, 1969), 700–721. For a limited attempt to investigate the linkage between congressmen and opinion leaders, see Anthony J. La Cavera, Sr., *Congressmen and Their Representative Constituents* (unpublished M.A. Thesis, University of Georgia, 1970).

[13] Throughout this paper the phrase "good assignments" is used as shorthand for a number of attributes which make a committee attractive to a congressman seeking to promote his re-election. Among the elements subsumed are the potential for service, promotion, or protection of some aspect of the economy of congressional districts, the possibilities of securing federal funds, and the likelihood of building a reputation in the district.

[14] Occasionally individual scholars have gained access to Democratic requests for single congresses. Randall B. Ripley, *Party Leaders in the House of Representatives* (Washington: Brookings Institution, 1967), pp. 59–61, analyzes Democratic requests for committee seats in the 88th Congress. Masters, "Committee Assignments . . ." presents data on Democratic committee requests for the 86th Congress which could come only from the request books. Richard F. Fenno, Jr., had access to both parties' request books for the 83rd-88th Congresses; see Fenno, *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown, 1966), p. 64.

[15] Masters, pp. 345–357; Clapp, *The Congressman*, pp. 207–240; William L. Morrow, *Congressional Committees* (New York: Scribner's, 1969), pp. 41–48; Richard F. Fenno, Jr., "Congressional Committees: A Comparative View," paper presented at the 69th Annual Meeting of the American Political Science Association, Los Angeles, California, September 8–12, 1970, pp. 2–11. The Fenno notion of "policy interest" can subsume the oft-cited factors of legislator experience and concern.

[16] The concept of an "interested" congressman as one whose constituency characteristics suggest particular concern about policy making in an area is taken from David R. Mayhew, *Party Loyalty Among Con-*

For most members some committees are so valuable that assignment to any one of them would be welcome. Among these are five which will be referred to as Top Committees—Appropriations, Rules, Ways and Means, Armed Services, and Foreign Affairs.[18] The first three are reputed to be the most prestigious in the House, and all five have been shown to cluster together at the top of scales empirically measuring committee desirability.[19] These committees are the anvils upon which many of the country's most important decisions are shaped. Because of the uniformly high prestige of these committees and their participation in important decision making, the small number of freshmen assigned to them have been lumped together for purposes of analysis.[20]

Top Committees offer two advantages which may help a congressman win re-election. First, the more aware citizens in a representative's district may note his elevation to one of these committees and be favorably impressed. One of Manley's interviewees reports, "I got more congratulatory telegrams from home after I was elected to Ways and Means than I did after I won election to Congress."[21] Second, the committee's subject matter responsibilities offer opportunities to promote district interests. Appropriations members can expedite funding of local projects and have easy access to federal bureaucrats whose budgets they review.[22] Appointees to Ways and Means can use member bills to alleviate the impact of tax laws on industries in their districts.[23]

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gressmen: The Difference Between Democrats and Republicans, 1947–1962 (Cambridge, Mass.: Harvard University Press, 1966).

[17] Various cut-off points have been used in classifying narrowly elected and safe legislators. Fenno and Manley each use 55 per cent in discussing members of the Appropriations and Ways and Means Committees; Fenno, *Power of the Purse,* p. 58; John F. Manley, *The Politics of Finance: The House Committee on Ways and Means* (Boston: Little, Brown, 1970), p. 51. Roger H. Davidson uses 60 per cent in *The Role of the Congressman* (New York: Pegasus, 1969), p. 127. Using an electoral margin of less than 55 per cent as the breaking point between narrowly elected and safe freshmen is not feasible in this paper since it would result in too few cases in the former category for analysis.

[18] This category is similar to one used in a study of Senate committees by Dwaine Marvick, *A Quantitative Technique for Analyzing Congressional Alignments* (unpublished Ph.D. dissertation, Columbia University, 1950), cited in Donald R. Matthews, *U.S. Senators and Their World* (New York: Vintage Books, 1960), pp. 154–155. Fenno in "Congressional Committees" (p. 5) reports that constituency service is not the primary motivation for members of Appropriations, Ways and Means, or Foreign Affairs. Nonetheless, it is a consideration for some members, e.g., "Most Appropriations members believe that there is no better committee from which to 'do more for the people back home.'" Elsewhere Fenno points out that Appropriations is prized particularly by men who have served on two of the most constituency-oriented committees; Fenno, *Power of the Purse,* p. 86. While a majority of the members on Top Committees may be attracted for other reasons, constituency service may be the primary consideration of those fearful of voter rejection. Manley, *Politics of Finance,* pp. 82–84, says that Ways and Means members view their committee as having both positive and negative consequences for re-election.

[19] Masters, "Committee Assignments . . . ," pp. 351–354. One technique for measuring committee prestige involves comparing transfers from the committee with transfers to it. The larger the ratio of transfers to to transfers from, the higher the committee's prestige. This method has been used by George Goodwin, Jr., *The Little Legislatures: Committees of Congress* (Amherst, Mass.: University of Massachusetts Press, 1970), p. 114; Morrow, *Congressional Committees,* pp. 42–43; Warren E. Miller and Donald E. Stokes, *Representation in the American Congress* (Prentice-Hall, forthcoming); Charles S. Bullock, III, *The Committee Assignments of Freshmen in the House of Representatives, 1947–1967* (unpublished Ph.D. dissertation, Washington University, 1968), pp. 33–35. Rank ordering on the basis of the proportion of a committee's members who have no other assignments produces slightly different results; see Louis C. Gawthrop, "Changing Membership Patterns in House Committees," *American Political Science Review,* 60 (June, 1966), 366–373. A ranking of the "most prestigious" House committees (without an explanation of how prestige was measured) is found in Thomas R. Dye and L. Harmon Zeigler, *The Irony of Democracy* (Belmont, Calif.: Wadsworth, 1970), p. 244. The rankings of Gawthrop and Dye and Zeigler do not exactly duplicate my Top Committees in their first five slots. Nonetheless four of these Committees are within their top five slots in every case. In addition to ranking these committees as the top five in their ability to attract members from elsewhere, Miller and Stokes report that they cluster together with the lowest proportion of freshmen and the fewest members who leave to accept other appointments.

[20] During the period studied, no freshmen were assigned to Rules, and only three Republican neophytes came to Ways and Means. Seventeen freshmen have sat on Appropriations, 24 on Armed Services, and 19 on Foreign Affairs.

[21] Manley, *Politics of Finance,* p. 55.

[22] Fenno, *The Power of the Purse,* pp. 82–88.

[23] Manley, pp. 78–84. See also John F. Manley, "The House Committee on Ways and Means: Conflict Management in a Congressional Committee," *American Political Science Review,* 59 (December, 1965), 927–939.

While we lack studies in depth of the rewards of Armed Services and Foreign Affairs assignments, their ability to attract members from many other committees[24] suggests that service on these committees is accompanied by prestige, power, and recognition, albeit less so than for members of Appropriations, Rules, and Ways and Means. Armed Services is useful in promoting congressional careers since its members can aid the economies of their districts by attracting and retaining military bases and by securing contracts for local military suppliers.[25] Appointment to Foreign Affairs may earn one electoral benefits to the extent that the member can utilize his position to attract ethnic support.[26]

Observations by Fenno, Manley, and Masters suggest, however, that at least for exclusive committees (Appropriations, Rules, and Ways and Means) prerequisites make it unlikely that narrowly elected congressmen, particularly marginal freshmen, will be selected.[27] Appointments to exclusive committees are generally contingent upon demonstrated competence, consistent norm adherence, willingness to work hard, and representation of safe districts, all of which favor congressmen who have completed a House apprenticeship.[28]

The sixth committee which has utility for any freshmen seeking to win the support of their constituents is Public Works.[29] The distribution of federal money for internal improvements by this committee provides members with a competitive advantage in obtaining funds for dams, harbor dredging, federal buildings and myriad other projects which can be pointed to during campaigns as tangible evidence of the incumbent's value to his public.

While virtually all congressmen would relish a seat on one of the Top Committees or Public Works, other assignments draw the attention of representatives with certain types of districts. For the committees on Agriculture, Education and Labor, Banking and Currency, Merchant Marine and Fisheries, and Interior, data permit the designation of congressmen whose constituency characteristics indicate an especial attraction to one of the committees.[30] For three of these special appeal committees, demographic variables were used to measure the appropriateness of particular assignments. Agriculture, it is assumed, will attract congressmen from rural districts.[31] The test group consists of narrowly elected freshmen from districts in which at least 10 per cent of the labor force is directly engaged in farm work.[32] Urban legislators are presumed to be drawn to the Banking and Currency Committee, which is concerned with the banking system and the housing industry.[33] Narrowly elected freshmen from districts at least 75 per cent urban comprise the test group for Banking and Currency. Congressmen representing large concentrations of blue-collar workers are thought perhaps to be eager to serve on Education and Labor.[34] The test group consists of the narrowly elected from districts in which 55 per cent or more of the employed hold blue-collar jobs.[35]

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[24] During the 80th through the 90th Congresses, Armed Services and Foreign Affairs each attracted 46 transfers while losing ten and nine members respectively to other committees; 11 of the 19 moved to exclusive committees.

[25] One observer suggests that the work of the Armed Services Committee is so oriented toward district betterment that its name should be changed to the Military Installations Committee or the Committee on Military Real Estate; Richard Bolling, *House Out of Order* (New York: Dutton, 1965), p. 89. See also Lewis Anthony Dexter, "Congressmen and the Making of Military Policy," in *New Perspectives on the House of Representatives*, ed. Robert L. Peabody and Nelson W. Polsby, 2nd edition (Chicago: Rand McNally, 1969), pp. 180–182. The career of the late congressman Mendel Rivers (D-S.C.). while perhaps atypical, demonstrates the extent to which an Armed Services member can attract defense money to his district. See "Mendel Rivers," *Newsweek*, January 11, 1971, p. 18. Barry S. Rundquist, "The House Seniority System and the Distribution of Prime Military Contracts," paper presented at the 70th Annual Meeting of the American Political Science Association, Chicago, Illinois, September 7–11, 1971, reports that junior Republicans, but not junior Democrats on Armed Services received a disproportionate share of prime military contracts in 1960.

[26] This is suggested by some of the findings of Julius Turner, *Party and Constituency: Pressures on Congress* (Baltimore: Johns Hopkins Press, 1952), Ch. 5. While the most frequent goal of Foreign Affairs members was the making of good public policy, some did see electoral payoffs associated with the committees; Fenno, "Congressional Committees," pp. 10, 72.

[27] Fenno, *Power of the Purse*, p. 58; Manley, *Politics of Finance*, p. 51; Masters, "Committee Assignments . . . ," pp. 353–354.

[28] Davidson, *Role of the Congressman*, pp. 130–134, reports that politicos and trustees, the representational role types favored for exclusive committees, tend to come from safe districts.

[29] The value of Public Works in winning the hearts of one's public is stressed by Mississippi's last moderate-to-liberal congressman; see Frank E. Smith, *Congressman From Mississippi* (New York: Capricorn Books, 1964). Also see Morrow, *Congressional Committees*, pp. 43–44; Fenno, *Power of the Purse*, pp. 83, 86; and James T. Murphy, "Partisanship and the House Public Works Committee," paper presented at the 67th Annual Meeting of the American Political Science Association, Washington, D.C., September 3–7, 1968.

[30] Various authors have alluded to the tendency to fill vacancies on Agriculture, Banking and Currency, Education and Labor, Interior, and Merchant Marine and Fisheries with legislators whose districts could benefit from this service. Masters, p. 354; Morrow, pp. 43–48; George B. Galloway, *History of the House of Representatives* (New York: Crowell, 1961), p. 82; Bolling, *House Out of Order*, p. 108; Goodwin, *Little Legislatures*, pp. 77–78. Fenno, "Congressional Committees," finds Interior members to be motivated chiefly by desires of constituency service. Education and Labor members, while requesting that committee most frequently because of an interest in making policy in that area, can, if they support organized labor, anticipate return of principle plus interest come reelection. Mayhew, *Party Loyalty Among Congressmen*, in discussing farm, urban, labor, and western issues, shows that interested legislators, regardless of party are in the vanguard of the proponents of legislation reported by the Agriculture, Banking and Currency, Education and Labor, and Interior Committees respectively. Whether the foremost goal of these legislators is constituency service or not, they can, and no doubt do, hold up their voting records with pride when they campaign for reelection. The paralleling of constituency interests and committee assignments is also dealt with in John H. Kessel, "The Washington Congressional Delegation," *Midwest Journal of Political Science*, 8 (February, 1964), 11–16.

For the two remaining committees geographic features were considered in determining whom to classify as an interested. The Merchant Marine and Fisheries Committee, with its potential for affecting the economies of districts with water related industries, is assumed to attract congressmen from coastal districts.[36] For this committee the test group is competitive freshmen from districts bounded by the Atlantic or Pacific Oceans, the Gulf of Mexico, or the Great Lakes. Interior, which is responsible for irrigation and reclamation projects, public lands, grazing and mineral rights thereon, mining, and Indian reservations is a frequent goal of congressmen from large Western districts, that is, those larger than 8,500 square miles.[37] Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, and Wyoming are considered to be western states for test purposes.[38]

As shown in Table 1, freshmen placed on the five committees for which constituency characteristics were isolated typically represent districts evincing the relevant feature. In six of ten cases interesteds received at least half of all assigments made to freshmen on these committees. In the case of the Interior and Insular Affairs Committee, the proportion of seats held by interesteds is small only because of the limited number of interesteds available for appointment. Eliminating those interested first-termers who were blocked from serving on Interior because a fellow member of their delegation was already there and those who were tapped for a Top Committee or Public Works, 81 per cent of the interested Democrats and 83 per cent of the Republicans went on the committee. (Similar considerations are inappropriate for other committees where a larger pool of interesteds existed.) Only among Republicans assigned to Education and Labor do interesteds seem not to be drawn to the appropriate committee, which is not surprising in light of the low levels of support for Republican candidates typically found among blue collar workers.

Having stipulated the 11 committees which may be useful to electorally troubled freshmen, we find six remaining committees which are less frequently coveted and are assumed to be of relatively little value to threatened neophytes.[39] These committees, which will be re-

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[31] Charles O. Jones, "Representation in Congress: The Case of the House Agriculture Committee," *American Political Science Review*, 55 (June, 1961), 358–367; Charles O. Jones, "The Role of the Congressional Sub-Committee," *Midwest Journal of Political Science*, 6 (November, 1962), 329–331. One author refers to members of the Agriculture Committee as "ambassadors" for specific commodities; Neil MacNeil, *Forge of Democracy* (New York: David McKay, 1963), pp. 161–62. Congressmen are so prone to perceive the Agriculture Committee as a farm pressure group that on one of the rare occasions in which a legislator has rejected a committee assignment, Shirley Chisholm, a New York City Democrat, refused appointment to Agriculture, judging it alien to the interests of her urban constituency.

[32] The criterion for agricultural districts used by Mayhew, *Party Loyalty Among Congressmen*, p. 17, is that ten per cent of the population be engaged in farm work. I have chosen ten per cent of the labor force engaged in agriculture because district labor force rather than population seems a more realistic measure. While ten per cent may seem a low figure to some, it must be recognized that many wives and young people who work on farms are not tabulated as part of the labor force. Moreover, in an agrarian district, many local businessmen are engaged in serving the needs of farmers.

[33] Mayhew, pp. 61–64, looked almost exclusively at housing legislation in his discussion of pro-urban roll call votes. This legislation passed through the portals of the Banking and Currency Committees.

[34] Mayhew, pp. 93–94.

[35] This is the same figure used by Mayhew, pp. 92–94, in distinguishing between labor and nonlabor districts.

[36] An indication of the utility perceived by some segments of coastal districts in having their congressmen on Merchant Marine and Fisheries is the contribution of $37,500 in campaign funds to Rep. Edward Garmatz (D.-Md.) in 1970. Interestingly, Garmatz was unopposed. *Atlanta Journal and Constitution*, September 20, 1970, p. 10-B.

[37] Bolling, *House Out of Order*, pp. 102–103; Fenno, "Congressional Committees," pp. 38–41.

[38] These are the states defined as western by Mayhew, p. 128. Much of the pro-western legislation considered by Mayhew had been reported by the Interior Committee.

[39] Scholars have found that these committees tend to cluster in the bottom third of prestige rankings. For sources see footnote 19. In 1959, three of these committees—District of Columbia, House Administration, and Post Office and Civil Service—had fewer Democratic applicants than seats available, suggesting their

Table 1. Percentage of Interested Freshmen Appointed to Five Special Interest Committees 80th–90th Congresses

Seats Held By	Ag.	M.M. & F.	B. & C.	Int.	E. & L.
Democrats					
Interesteds	57	71	78	45	47
Others	43	29	22	55	53
	100	100	100	100	100
No. of Appointees	21	17	36	38	36
Republicans					
Interesteds	66	59	50	42	35
Others	34	41	50	58	65
	100	100	100	100	100
No. of Appointees	29	39	32	36	31

ferred to as Duty Committees,[40] are District of Columbia, Government Operations, House Administration, Post Office and Civil Service, Un-American Activities (now called Internal Security), and Veterans Affairs.[41] The Masters-Clapp hypothesis leads to the proposition that narrowly elected freshmen will be underrepresented on Duty Committees since such assignments will contribute little toward re-election.

The hypothesis that good assignments go to those in electoral need does not assume the exclusion of other factors. Nor does the specification of various constituency characteristics presuppose that there are no other items associated with one's district which may determine one's committee preferences. Nor, finally, does the author imply that constituency attributes are the only factors which motivate freshmen requests or assignment decisions. What is anticipated is that if Masters and Clapp are correct, good seats will be more often given to the electorally marginal than to the electorally secure.

Subjects and Method

In this study 830 committee assignments for 688 freshmen arriving on Capitol Hill between 1947 and 1967 were inspected. Of these, Northern Democrats account for 317 freshmen and 372 assignments; Republicans constitute the remainder. There were 366 narrowly elected freshmen with 435 assignments who were candidates in the next general election. Northern Democrats comprise 189 of these freshmen, holding 215 of the committee seats. While a few Southern Democrats[42] represented marginal districts, all were excluded; since, during the 11 congresses, only three of the 125 Southern Democratic freshmen were unseated by Republicans. The absence of serious bipartisan electoral competition makes it difficult to argue that committee assignments hold the key to re-election for many Southerners.[43]

Since we are concerned with the relationship between elections and committee assignments, freshmen who received initial appointment to two committees will be treated separately for each assignment unless otherwise stated. This strategy was adopted since it is impossible to determine the separate impact of each assignment on re-election chances.

Census data for 1950 and 1960 were used to measure anticipated interest in assignment to Agriculture, Education and Labor, and Bank-

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lack of attractiveness. Merchant Marine and Fisheries was the only other established committee for which supply exceeded demand. (There were also fewer requests than slots on Science and Astronautics, but this fact must be discounted since 1959 was the first year in which it was a standing committee); Masters, "Committee Assignments . . . ," p. 346. Three of these committees—District of Columbia, House Administration, and Un-American Activities—are generally considered unimportant by congressmen. See Raymond E. Wolfinger and Joan Heifetz, "Safe Seats, Seniority, and Power in Congress," *American Political Science Review*, 59 (June, 1965), 339. Long-time House staff member D. B. Hardeman reports that staffers generally regard the Duty Committees as unattractive. Hardeman classifies Government Operations, House Administration, District of Columbia, Veteran's Affairs, and Un-American Activities as "Bobtail Committees" and places Post Office and Civil Service last among the "Most Sought After Committees"; see Goodwin, *Little Legislatures*, p. 116. Furthermore all six committees are among the nonexclusive ones; Goodwin, p. 68. While these committees are unattractive to most legislators, some have an appeal for certain types of congressmen. The best example of this atypical allure is the District of Columbia Committee which because of its control over black-populated Washington is popular with segregationist Southerners. Masters, p. 335; Bolling, *House Out of Order*, p. 105. Post Office and Civil Service is viewed as constituency-serving by some of its members, but the majority of them did not request the assignment and abandoned it as soon as possible; Fenno, "Congressional Committees," pp. 73–74.

[40] This category is adapted from that used by Marvick, *Quantitative Technique. . . .*

[41] Three committees are excluded from this study: Science and Astronautics is omitted because it was not created until 1959; the Judiciary Committee is not dealt with because of its unique requirement that members be attorneys; the Interstate and Foreign Commerce Committee was omitted because of an inability to define a constituency characteristic associated with committee service.

[42] Southern Democrats are from 11 states of the Confederacy.

[43] Another reason for excluding Southern Democrats is the small number of appointments to some committees made to this group.

ing and Currency. Data from the 1950 census were used for freshmen in the 80th through the 84th Congresses; 1960 data were used for the remainder of the period studied. When possible, material in the *Congressional District Data Book*[44] and compilations in *Congressional Quarterly*[45] were used. When data were available nowhere else, figures were computed from county enumerations. No attempt was made to compile data for districts with boundaries not coterminous with county lines. Districts in cities, e.g., Chicago, Philadelphia, and New York, were assumed to be highly urban and low in agricultural employment, without actual calculations being made.

Hypothesis testing is accomplished by using chi square when the total N exceeds 50 and no cell has fewer than five cases.[46] Fisher's exact test is used for smaller N's.[47] In either case the null hypothesis is rejected only if the findings are significant at the .05 level.

Narrowly Elected Freshmen and Good Assignments

The null hypothesis tested in this section is: Narrowly elected and safe freshmen are given "good" assignments at the same rates. Rejection of the hypothesis and evidence that narrowly elected freshmen are more likely to draw a particular assignment would provide support for the Masters-Clapp hypothesis.

In looking at appointments to the Top, Duty, and Public Works committees, the assignments of all freshmen were examined, and each freshman was categorized as coming from either a safe or marginal district. For these committees there is only one instance (see Table 2) in which the null hypothesis can be rejected, and it does not support Masters-Clapp. For Republicans, assignments to the Top Committees are associated with the electoral character of one's district, however, legislators from safe, not marginal, districts are disproportionately the beneficiaries.

In evaluating assignments to Agriculture, Banking and Currency, Education and Labor, Interior, and Merchant Marine and Fisheries, only interesteds were studied. Since an unwritten rule of the House limits all but the largest delegations to a single appointee per committee, freshmen in delegations already represented on a committee were excluded even though their constituency characteristics suggest an interest in a particular assignment. Also excluded were interested freshmen seated on a Top Committee or Public Works, since these appointments may be preferred to serving on a special interest committee.

Dividing the interested freshmen for each committee into safe and marginal categories, a significant difference occurred only for the Democratic appointees to Education and Labor. Examination reveals, however, that safe rather than marginal Democrats are overrepresented on the committee. Only among Democrats assigned to Agriculture and Public Works was there even a moderate though not statistically significant overrepresentation of the narrowly elected. Thus the data do not support the Masters-Clapp generalization.

The two instances in which a significant relationship exists between marginal elections and initial assignment, while contradicting the reelection promotion hypothesis, lend support to alternative explanations of committee assignments. Republican hesitancy to entrust insecure freshmen with Top Committee positions may be attributable to considerations noted earlier which reduce the likelihood of placing freshmen and representatives from marginal districts on exclusive committees.[48] While not selecting

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[44] Bureau of the Census, *Congressional District Data Book (Districts of the 88th Congress)* (Washington: U.S. Government Printing Office, 1963), Supplements to this volume were used for districts redrawn since 1963.

[45] *Congressional Quarterly Weekly Report* (June 20, 1956).

[46] Some would argue that since the universe of all freshmen over a 20-year period and not a sample thereof is dealt with, significance tests are inappropriate. This stance is taken in, for example, John C. Wahlke, Heinz Eulau, William Buchanan, and LeRoy C. Ferguson, *The Legislative System* (New York: John Wiley, 1962), pp. 455–458. On the other hand Hubert M. Blalock, *Social Statistics* (New York: McGraw-Hill, 1960), pp. 270–271, suggests that tests of significance "will practically always be helpful in evaluating one's findings." The use of statistical significance to place findings in some kind of perspective even when the researcher has the universe rather than a random sample has been frequent, particularly in recent policy studies using the state as a unit of analysis. A few representative examples include: Thomas R. Dye, *Politics, Economics, and the Public* (Chicago: Rand McNally, 1966); Ira Sharkansky, "Government Expenditures and Public Services in the American States," *American Political Science Review*, 61 (December, 1967), 1066–1077; Richard I. Hofferbert, "Ecological Development and Policy Change," *Midwest Journal of Political Science*, 10 (November, 1966), 464–483. Since the present paper engages in hypothesis testing, a commonly accepted level of statistical acceptance, .05, rather than an arbitrary percentage difference, was used as a rule of thumb.

[47] These are the standards suggested for choosing between chi square and Fisher's exact test by Dennis J. Palumbo, *Statistics in Political and Behavioral Science* (New York: Appleton-Century-Crofts, 1969), p. 156.

[48] Masters, "Committee Assignments . . . , pp. 351–354; Manley, *Politics of Finance*, p. 51; Fenno, *Power of the Purse*, p. 58.

Table 2. Differences in the Rates at Which Narrowly Elected and Safe Freshmen Were Given Good Committee Assignments

	House		Education and Labor Committee Dems.		Reps.	
	On E & L	Elsewhere	On E & L	Elsewhere	On E & L	Elsewhere
Narrowly Elected	8	38	4	17	4	21
Safe	20	34	13	12	7	22
	$X^2 = 3.381$ $p < .10$		Fisher's Exact Probability = .02		Fish. = .35	
			Top Committees			
	On Top Comm.	Elsewhere	On Top Comm.	Elsewhere	On Top Comm.	Elsewhere
Narrowly Elected	30	405	23	192	7	213
Safe	33	362	14	143	19	219
	$X^2 = 0.437$ $p < .70$		$X^2 = 0.153$ $p < .90$		$X^2 = 4.066$ $p < .05$	

	House	Dem.	Rep.
Interested in Agriculture*	$x^2 = 0.969$ $p < .40$	Fish. = .11	$x^2 = 0.006$
Interested in Banking and Currency	$x^2 = 0.060$ $p < .90$	$x^2 = 0.409$ $p < .70$	Fish. = .42
Interested in Merchant Marine and Fisheries	$x^2 = .025$ $p < .90$	$x^2 = 0.041$ $p < .95$	Fish. = .39
Interested in Interior	Fish. = .29	Fish. = .30	Fish. = .67
Interested in Public Works	$x^2 = 1.063$ $p < .40$	$x^2 = 2.838$ $p < .10$	$x^2 = 0.006$ $p < .95$
Assigned to Duty Committees	$x^2 = 1.010$ $p < .40$	$x^2 = 0.001$	$x^2 = 1.447$ $p < .30$

* Similar procedures were carried out for freshmen interested in appointment to Agriculture, Banking and Currency, Merchant Marine and Fisheries, Interior, and Public Works. Since significant differences in the rates at which marginal and safe freshmen were appointed to these committees were absent, only the chi squares or Fisher's exact probability are reported. This has been done to conserve space. Those who are interested can receive copies of the tables used in calculating the statistics from the author.

many first-termers, Democrats differ from Republicans by not penalizing freshmen from marginal districts. Partisan differences may be a product of majority-minority status with the minority party less willing to risk giving its limited number of Top Committee assignments to congressmen most susceptible to defeat.

Recruits for Education and Labor, like appointees to exclusive committees, are reputed to be from safe districts.[49] Since the committee has been confronted with some of the most controversial topics to come before the postwar Congress, party leaders have often sought men who could be counted on to maintain partisan positions without equivocation. Therefore, Fenno explains, the new recruits have been men who could enter the fray without having to temper their behavior to appease their constituents.

An additional finding is the inability to reject the null hypothesis for Duty Committees. That narrowly elected and safe freshmen are equally likely to begin service on these committees is

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Masters, "Committee Assignments . . . ," pp. 354–355; and John S. Saloma, III, *Congress and the New Politics* (Boston, Little, Brown, 1969), pp. 117–126.

[49] Richard F. Fenno, Jr., "The House of Representatives and Federal Aid to Education," in *New Perspectives on the House of Representatives*, ed. Robert L. Peabody and Nelson W. Polsby, 2nd ed. (Chicago: Rand McNally, 1969), pp. 283–323. In-fighting on Education and Labor is also discussed in MacNeil, pp. 172–173; Bolling, *House Out of Order*, pp. 96–97;

further evidence that the Masters-Clapp hypothesis is inaccurate. Coming from a marginal district does not lessen one's chances of appointment to a Duty Committee.

Since some congressional districts had characteristics which suggest that more than one interest committee might be an appropriate assignment, focusing on assignments rather than congressmen may account for the inability to reject the null hypothesis. When, however, congressmen are used as the units of analysis and representatives given good assignments are compared with those assigned elsewhere, it does not appear that the narrowly elected are treated better than freshmen from safe districts. Indeed narrowly elected Democrats are slightly less likely to obtain good appointments than are other freshmen of their party. (See Table 3.)

The total absence of support for the proposition that the narrowly elected receive better committee posts than do their peers from safe districts suggests that other considerations largely supplant re-election promotion in the compilation of committee rosters. Masters and Clapp have overemphasized the importance attached to giving electorally beneficial assignments to those endangered by the opposition party. Masters and Clapp may have come to an erroneous conclusion because they lacked representative samples. Masters does not indicate those to whom he talked, but the participants in the round table discussions upon which Clapp relied tended to be young and liberal.[50] Without a random sample, Masters and Clapp may have mistakenly generalized from the perceptions of the congressmen whom they interviewed. A second explanation may be that interviews by Clapp and Masters tapped norms, while I am reporting actual behavior.

Table 3. Number of Congressmen Receiving Good Committee Assignments Controlling for Party and Electoral Competitiveness

	Democrats Narrowly Elected		Safe	
	No.	%	No.	%
Good Assignments	84	45	62	47
Other Assignments	103	55	69	53
	187	100	131	100

$X^2 = .096$

	Republicans Narrowly Elected		Safe	
	No.	%	No.	%
Good Assignments	71	40	79	40
Other Assignments	105	60	118	60
	176	100	197	100

$X^2 = .003$

Committee Assignments and Re-election

A crucial question remains: Is the belief that a certain assignment can promote re-election valid? If not, the assumption underlying Masters' and Clapp's generalization is faulty and it is not surprising that freshmen from marginal districts are frequently passed over in filling desirable vacancies. In this section the electoral consequences of committee assignments for marginal freshmen are investigated by comparing the re-election rates of members having good assignments with those for recipients of other appointments. Only freshmen who won less than 55 per cent of the vote in their first election are considered, since committee assignments would seem to be less determinative of the fortunes of men from safe districts.[51]

The small number of cases on some of these committees dictate caution in interpretation. Nevertheless material in Table 4 fails to produce a single statistically significant difference in the re-election rates of marginal freshmen depending upon their committee assignments, although there is an indication that in the House as a whole, as well as for Democrats, there is some relationship between assignment of interesteds to Interior and their likelihood of re-election.[52] Rarely do committee assignments seem to help the narrowly elected win second terms. This conclusion draws additional support from the finding that marginal novices initially assigned to Duty Committees were about as likely to be re-elected as were freshmen assigned elsewhere.

Although the narrowly elected who received good assignments are not returned to Congress at rates significantly different from those of marginal novices assigned elsewhere, perhaps the former poll a larger share of the vote when seeking re-election than do the latter. It may be that certain assignments help freshmen from swing districts win votes, even if the increment

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[50] Clapp, *The Congressman*, p. 7.

[51] Only 10 per cent of safe Republican and five per cent of safe Democratic freshmen failed in re-election bids.

[52] Interestingly, of the six committees studied by Fenno, Interior is one of the two which members perceive as offering primarily opportunities for constituent service; Fenno, "Congressional Committees," pp. 38–41.

Table 4. Differences in the Re-election Rates of Narrowly Elected Freshmen, Comparing Those Given Good Assignments and Those Placed on Other Committees

	Interior Committee					
	House		Dems.		Reps.	
	Re-elected	Defeated	Re-el.	Def.	Re-el.	Def.
On Interior	19	1	11	0	8	1
Elsewhere	28	9	18	5	10	4
	Fisher's Exact Probability = .07		Fisher's = .12		Fisher's = .33	

	House	Dems.	Reps.
Interested in Agriculture**	$x^2=0.015$ $p<.80$	*	Fish. = .52
Interested in Banking and Currency	$x^2=0.014$ $p<.80$	Fish. = .65	Fish. = .74
Interested in Merchant Marine and Fisheries	Fish. = .63	*	Fish. = .69
Interested in Public Works	$x^2=0.984$ $p<.40$	$x^2=0.008$ $p<.90$	$x^2=1.541$ $p<.30$
Interested in Top Committees	$x^2=0.565$ $p<.50$	$x^2=0.006$ $p<.95$	*
Assigned to Duty Committees	$x^2=0.975$ $p<.50$	$x^2=0.027$ $p<.90$	$x^2=2.088$ $p<.20$

* Fewer than nine narrowly elected interesteds assigned to these committees sought re-election. No data are presented on narrowly elected interesteds placed on Education and Labor because the number of cases is too small.

** To conserve space, since there were no significant differences in re-election rates depending upon committee assignments, only the chi squares or Fisher's exact probabilities calculated from similar tables for other committees are included. Those wishing to inspect the tables from which these statistics were computed should contact the author.

is inadequate for re-election. Data in Table 5, however, give little indication that any particular assignments are electorally rewarding. Excluding Duty Committees, the mean vote received in re-election contests was higher for freshmen with good assignments in only three instances. The most notable instance—rural Republicans on Agriculture—is distorted by one aberrant case, a Californian who after winning 53.1 per cent of the vote in 1947 secured the nominations of both parties in 1949 under the now eliminated cross-filing process. Excluding this case, the mean re-election vote received drops to 56.8 per cent, lower than the average for those holding other assignments. The other instance in which a difference of more than two percentage points was found occurred among Democratic interesteds on Interior.

Thus committee assignments do not appear to figure prominently on the bids of narrowly elected freshmen for sophomore terms. Between 1948 and 1968, 71 per cent of the freshmen from competitive districts who ran won in the general election. While this proportion is substantially less than either the percentage of safe freshmen (92 per cent), or of all incumbents (94 per cent)[53] who were re-elected, it indicates that even the most vulnerable set of congressmen is typically victorious. Losses suffered by narrowly elected freshmen are concentrated in those classes which had been affected by abnormally strong short term electoral forces.[54] Table 6 shows that 92 per cent of the

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[53] The proportion of incumbents re-elected varies depending upon which years are used; nonetheless, re-election rates rarely slip below 90 per cent. Between 1954 and 1960 an average 83.9 per cent of the House incumbents were returned in the general election. Charles O. Jones, "The Role of the Campaign in Congressional Politics," in *The Electoral Process*, ed. M. Kent Jennings and L. Harmon Zeigler (Englewood Cliffs, N.J.: Prentice-Hall, 1966), p. 24. Between 1954 and 1964, omitting 1962, 92 per cent of the incumbents seeking re-election were victorious; Barbara Hinckley, "Seniority in the Committee Leadership of Congress," *Midwest Journal of Political Science*, 13 (November, 1969), 620. In 1962 and 1966, 94 and 89 per cent respectively of the incumbents successfully turned back challenges; Charles O. Jones, *Every Second Year* (Washington: Brookings Institution, 1967), p. 68.

[54] For a discussion of short term-electoral influences see Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley, 1966).

Table 5. Mean Percentage of the Two-Party Vote Won by Narrowly Elected Freshmen Controlling for Committee Assignment

Assigned To	% Vote First Election	% Vote in Re-election Bid	N
	Democrats		
Top Committees	52.3	53.9	22
Elsewhere	52.0	53.6	176
Public Works	51.9	51.9	17
Elsewhere	52.0	53.8	181
Agriculture	51.6	49.4	7
Elsewhere	52.0	52.3	41
Interior	52.5	58.8	11
Elsewhere	51.9	56.7	23
Merchant Marine and Fisheries	51.8	54.8	5
Elsewhere	51.9	55.6	75
Banking and Currency	51.9	54.5	12
Elsewhere	51.9	55.2	68
Duty Committees	51.9	53.8	60
Elsewhere	52.1	53.4	138
	Republicans		
Top Committees	53.5	54.5	6
Elsewhere	52.4	54.9	196
Public Works	51.8	52.8	18
Elsewhere	52.5	55.1	184
Agriculture	52.3	61.0	10
Elsewhere	54.2	58.3	36
Interior	52.3	58.8	9
Elsewhere	52.1	61.1	14
Merchant Marine and Fisheries	52.8	52.5	10
Elsewhere	52.5	53.8	59
Banking and Currency	52.6	51.3	10
Elsewhere	52.4	53.1	72
Duty Committees	52.3	54.1	66
Elsewhere	52.5	55.3	136

narrowly elected Democrats who lost re-election bids had been elected in the Democratic sweeps of 1948, 1958, and 1964. Two-thirds of the defeats suffered by Republican freshmen from competitive districts occurred in the large classes of 1946 and 1952. Another 13 per cent of the defeated Republicans were defenseless victims of the Goldwater folly. Regardless of the assignment received, all but five of the Democrats and nine of the Republicans winning close initial elections in years other than those mentioned above won their next general election.

The fate of the narrowly elected seems rarely to be linked with committee assignments. Defeat most frequently occurs as a repercussion of events at the national rather than the district level. Good works on committee and constituency service seem insecure moorings amidst the ebb and flow of national electoral trends.

The present findings are in accord with evidence that voters are largely unaware politically, responding neither to policy alternatives nor to knowledge of the caliber of performance rendered by elected officials. In explaining their preferences in the 1958 congressional elections, only three per cent of those polled mentioned issues.[55] If congressmen's issue stands are of little consequence to voters, committee assignments, being less widely known, will be of even less importance. Even the suggestion made earlier that despite mass ignorance, the response of local elites who might be aware of committee assignments and performance could be decisive in elections receives no support from data presented here.

The findings of this paper suggest that congressmen have more latitude in role selection than is often thought. Even the narrowly elected need not rank constituency promotion and service foremost when seeking committee appointments. Most congressmen may be free to make committee requests on the basis of subject-matter interests, political ambitions, desires for power, and other goals which may or may not be perceived to improve their standing with their constituents. Congressmen may be able to pay proper homage to the Madisonian ideal of constituency interest representation by processing casework, visiting the district, expressing concern for district problems, and making a display of their attempts to resolve such problems.[56] These actions are less de-

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[55] Miller and Stokes, "Constituency Influence . . . ," p. 54. The same study also found that only 49 per cent of the electorate had heard or read anything about the incumbent prior to the 1958 congressional elections. Dexter quotes a congressman as saying, "I never heard of any man being beaten on an issue," Lewis Anthony Dexter, *The Sociology and Politics of Congress* (Chicago: Rand McNally, 1969), p. 2. This is, of course, an overstatement, but it may express an evaluation which is far more accurate than not.

[56] Dexter, pp. 2–3, 79. The finding that the narrowly elected placed on Duty Committees are not appreciably handicapped can perhaps be explained in that Duty assignments require less time and attention al-

Table 6. Distribution of Narrowly Elected Freshmen and Narrowly Elected Freshmen Losing Re-election Bids by Year*

Initial Election	Democrats				Republicans			
	Narrowly Elected Freshmen		Narrowly Elected Fr. Defeated		Narrowly Elected Freshmen		Narrowly Elected Fr. Defeated	
	N	%	N	%	N	%	N	%
1946	4	2	0	0	29	17	21	47
1948	35	19	18	31	5	3	0	0
1950	2	1	0	0	15	9	1	2
1952	13	7	2	3	22	13	9	20
1954	14	7	2	3	9	5	1	2
1956	10	5	0	0	11	6	5	11
1958	37	20	17	29	12	7	0	0
1960	14	7	1	2	21	12	2	4
1962	14	7	0	0	15	9	6	13
1964	41	22	19	32	8	5	0	0
1966	3	2	0	0	28	16	0	0
Totals	187	99%**	59	100%	175	102%**	45	99%

* Only congressmen defeated in the general election are shown. It is assumed that if the incumbent lost the primary it was due not to his committee assignment, but to other factors.

** Totals do not equal 100 per cent because of rounding.

manding than what some—and perhaps most—marginal congressmen believe to be necessary in order to win re-election. Moreover these tasks can be largely handled by staff members, thus allowing congressmen more time for other matters.

A final observation suggested by the data is that actors important in making committee assignments have great leeway in positioning narrowly elected freshmen. Since assignments do not condition re-election, the committees on committees can typically ignore pleas that a particular assignment is essential to the career of a new member. If members of the committees on committees have reached this conclusion, it would explain the finding that the narrowly elected do not receive a disproportionate share of the good assignments.

Summary

Data have been presented to test the hypothesis that initial committee assignments are awarded so as to promote the re-election of congressmen from marginal districts. There was no evidence to support the conclusions of Charles Clapp and Nicholas Masters that narrowly elected freshmen receive preferential treatment from the committee on committees. The few instances in which there was a significant difference in assignment patterns stemmed from overrepresentation of safe—not marginal—first-termers. The absence of support for the Masters-Clapp proposition was complemented by the finding that there were no significant differences in re-election rates associated with the kinds of assignments marginal freshmen received. Instead, re-election defeats of narrowly elected freshmen coincide with national electoral trends.

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lowing the congressman to concentrate more heavily on casework, and so on.

COMMUNICATIONS

TO THE EDITOR:

Based on its early voting studies, the Survey Research Center suggested that "only a modest articulation between party policy and voter response" exists. Political scientists have interpreted these studies to mean that issues generally don't count for much as the collective American voter makes up his mind. But political scientists have never found themselves very comfortable with that interpretation. A host of articles and books have attempted to revise our understanding of the role of issues in electoral choice. Perhaps the most influential of these was V. O. Key's unfinished little book, *The Responsible Electorate.*

An important breakthrough is found in David RePass's article, "Issue Salience and Party Choice" (*American Political Science Review,* June 1971). This article is one of the first to come to grips with measurement problems while still using data sets similar to the early SRC studies. It nicely illustrates Merton's discussion of the manner by which new methods of empirical research can lead to the reformulation of theory. In this case, the use of questions better suited to measure salience (on the respondent's own terms) leads to the conclusion that ". . . the voting public has at least a few substantive issues in mind at the time of election, and the voters seem to be acting more responsibly than had previously been thought" (p. 400). The study shows that on most issues mentioned by the respondents, well over a majority of the voters do "perceive party differences on those issues salient to them" (p. 394), and despite some distortion to make the perceptions of party issue stances consistent with personal party identification, there is "a strong strain toward correct perception of party positions" (p. 395).

At the climactic point, however—where "the role of issues in individual voting choice" is assessed—the article is subject to serious misinterpretation (as evidenced by panelists' claims and corridor talk at recent professional meetings). In large part, this stems from confusion in reading his difficult Table 6 (p. 399), but also from RePass's own unfortunate use of language, when he says:

> Table 6 shows the *powerful* [italics mine] influence of issue partisanship (or issue alignment) on voting choice. When issue partisanship conflicted with party identification, the issues often overcame the long-term party loyalties; the stronger the issue partisanship, the greater its electoral effect. The bottom and top rows of Table 6 are especially worthy of note; if several salient issues intervened, party identification had virtually no influence on voting choice.

RePass, of course, is addressing the same kind of question Key raised, albeit through different concepts and measures: do voters switch from one party to another because of issues? Key found that of those who voted in the previous election, anywhere between 11 per cent and 22 per cent, depending on the election year, switched and these "switchers" offered "issue" grounds for their behavior. RePass uses the party identification of the voter in the *current* election as his baseline for determining issue pull and, in so doing, is probably on firmer grounds conceptually. Yet, if the same data in Table 6 are reformatted and presented in an easily readable manner, one comes away from the study with a much different conclusion and research strategy: (1) *issue partisanship does not exert a very powerful pull toward the opposition party* and (2) if we want to study "rational" and "responsible" voters we should pay more attention (a) to those *who stay within their party for issue reasons* and (b) to *Independents.*

RePass's Table 6 is presented below. Be-

Table 6. Direction of Vote by Strength of Issue Partisanship Controlling for Party Identification

Strength of Issue Partisanship	Party Identification				
	Strong Dem.	Weak Dem.	Ind.	Weak Rep.	Strong Rep.
High Dem	2% (55)	4% (22)	9% (11)	0% (3)	—
Med Dem	1% (77)	2% (41)	4% (28)	20% (10)	40% (5)
Low Dem	0% (83)	11% (63)	8% (40)	18% (28)	33% (6)
No Difference	10% (37)	21% (57)	22% (45)	56% (39)	94% (16)
Low Rep	20% (10)	45% (20)	58% (33)	77% (31)	83% (17)
Med Rep	33% (3)	62% (13)	81% (16)	93% (14)	96% (27)
High Rep	100% (5)	89% (9)	93% (27)	90% (20)	99% (71)

The entry in each cell is the per cent of the voters in the cell who voted Republican.

cause each cell entry is "the per cent of the voters in the cell who voted Republican" one must transpose the data in the two Republican columns to their complement to get at "switchers."

One notes immediately that the N's in the bottom and the top rows are very small (e.g., 2 per cent of 55 equals 1 voter). To get these cells into interpretable sizes and still remain true to the intent of RePass's test, I have collapsed the three "strength of issue partisanships" rows for each party into a single row for each party. The results are presented above.

Individual Voting Change as Measured by Cross-Over Voting (i.e., Percentage of Voters Pulled Away from Party I.D. by the Issues)

Direction of Issue Partisanship	Strong Dem.	Weak Dem.	Weak Rep.	Strong Rep.
	a		b	
Dem	1% Rep (215)	7% Rep (126)	80% Dem (41)	64% Dem (11)
	c		d	
Rep	44% Rep (18)	60% Rep (42)	15% Dem (65)	4% Dem (115)

Now that the N's are interpretable we can see that we are working with figures like 44 per cent through 80 per cent who are "switchers" rather than the more impressive 100 per cent figures.

Secondly when we compare the complement of the N's and percentages for the a and d diagonals ("standpatters") with the N's and percentages for the b and c diagonals ("switchers"), we get a strong hint of where to look for a payoff in studying issues. There are 485 standpatters; of those who felt an issue pull *toward* their own party, 93 per cent voted for it. However, there are only 73 switchers; of those who felt an issue pull *away* from their own party, only 65 per cent voted for the opposition. The former grossly outnumber the latter, and *they appear to have good issue reasons for staying with their party.*

To conclude the point, and here we are moving beyond the RePass article to a direct test of Key's question through the RePass data, we note that crossover voting is not a very good place to document the pulling power of issues. For, if we define "rational" voters in Key's terms, they would be the individuals in cells b and c. These are party identifiers who appear to have crossed over to the opposition because of issues. Of the respondents who offer party identification in the original table, 9 per cent are "rational" cross-over voters. Curiously enough, when we look at cells a and d, we find that 3 per cent are totally "irrational," that is, the issues favored their own party, yet they *still* crossed over and voted for the opposition. The net pulling power of 6 per cent suggests that we had better scale down our claims about the "powerful influence of issue partisanship" or, better yet, look to the more fruitful area where issues may exert very powerful influence in *keeping people within their own party.* RePass suggests this point on pages 394–395 and hints at it in his multiple regression analysis on page 400, but fails to discuss its full significance.

Furthermore, when one considers that 20 per cent of those listed in the original Table 6 were Independents and about 85 per cent of the Independents who perceived issue differences between the parties voted consistent with the issue pull, they too represent a more fruitful area to document the effects of issues on individual voting choice. Again the author hints at the point on pages 398–399 but does not develop it fully.

RePass has taken us a long way. But with some additional analysis of his data we are led to suggest modifications in his findings and a new direction for future research—the effects of issues on "standpatters" (rather than "switchers") and on Independents. Public knowledge usually has within itself the elements for its own reformulation.

David C. Leege
University of Illinois, Chicago Circle

To the Editor:

If my article "Issue Salience and Party Choice" has been subject to misinterpretation, I am happy to have the opportunity to clarify its findings. David Leege discusses two major points in his letter: one having to do with my measure of issue partisanship and its effect on pulling voters to the opposite party, and the other having to do with the study of voter "rationality." Let me deal with the second point first.

Nowhere in my article did I use the word "rational"; indeed I carefully avoided using that term. The article does not present sufficient evidence to evaluate voter rationality. It does show that most voters have some specific issues in mind and that these issues do have an impact on electoral choice. To be sure, these findings give encouragement to those of us who have been concerned about the quality of the electoral decision, but is is premature to conclude that the American voter is "rational" on the basis of data so far published.

"Rationality" is a very complex concept, and any satisfactory definition of it would have to include a variety of factors, not just issue awareness. Degree of informational support, correctness of party choice (given an issue or goal), and objectivity are some of the factors that one would want to consider in evaluating whether or not voters were "rational." V. O. Key used the term "rational" quite frequently in *The Responsible Electorate,* but he failed to give it any systematic meaning. He seemed to imply that those voters, particularly the "switchers," who chose on the basis of policy or issues were "rational." This is only a partial—a very incomplete—definition of "rationality."

It should also be pointed out that a voter may be candidate-oriented and still be "rational." If a voter has studied the *candidates'* positions on some issues or has tried to evaluate critically the *candidates'* qualifications for office, he could be considered a "rational" voter even if he indicated no particular issue concerns when he was interviewed. But apparently Key's argument in *The Responsible Electorate* has such a grip on the minds of some political scientists that they presume that issue voting is the *only* basis for "rationality." Note that Leege implies that the 3 per cent who voted for the opposition even though "issues favored their own party" were "irrational." Could it be that these 3 per cent had scrutinized both candidates and voted for the opposite candidate for good reason, or had some other valid reason for voting the way they did?

Some time before the "Issue Salience and Party Choice" article was published, I had already begun a research project in which I set out to develop an operational definition of "rationality" that could be applied to existing SRC data. I have since arrived at such a definition and have applied it to the SRC 1964 election data. I have personally read all of the verbatim responses from the microfilmed 1964 protocols—including the responses to open-ended questions on parties and candidates as well as issues—and evaluated them in terms of the criteria I had established. I hope to be able to present some findings from this contextual analysis in the near future. In the meantime, suffice it to say that I was not satisfied that I had found "rationality" in the data I presented in "Issue Salience and Party Choice," and that is why I went back to the original interviews to probe the subject further.

I agree in principle with Leege when he says that ". . . if we want to study 'rational' and 'responsible' voters we should pay more attention (a) to those *who stay within their party for issue reasons* and (b) to *Independents.*" I would go further and advise that, instead of studying selected categories of voters, we make individual-level observations on *all* of the voters. I now have such individual-level measures for the 1964 data and will be able to describe the "levels of rationality" among the American electorate when my analysis is completed.

Now, to discuss Leege's comments concerning the importance of issue partisanship on voting choice, particularly his concern about Table 6. There seem to be two reasons for this desire to reconstruct Table 6 in the way he did. One, the table was too "difficult" as it stood; and two, he wanted to cast my data into Key's mode of analysis, i.e., into "switchers" and "standpatters." The first reason is trivial, although if political scientists around the country are confused by a three-dimensional percentaged table, one might begin to wonder how many readers are getting anything out of the really complex pieces of quantitative analysis now published in profusion. There is no doubt that Leege well understood the table; he did a thorough job of decomposing it (with a minor error—there are 495, not 485, "standpatters"). Also, I am sure that if Leege had thought a moment, he would not have implied that the upper left corner cell was *based* on only one case—it is based on an N of 55 cases.

But these minor matters aside, there is a reason for concern when a political scientist of Leege's reputation wants to dismember a perfectly good table like Table 6. Table 6 is based on my "Strength of Issue Partisanship" measure—a measure which varies from a very strong degree of Democratic partisanship, through a neutral point, to a very strong degree of Republican partisanship. (This measure is collapsed to a seven-point scale in Tables 5 and 6.) A very strong degree of Democratic issue partisanship meant that a respondent mentioned several issues, that the issues were quite salient to him, and that he thought that the Democratic party would do what he wanted on all of them. Someone who mentioned several issues but who did not perceive party differences on any of them would have landed in the neutral category. Someone who mentioned three problems and felt that the Democrats would do what he wanted on one, but perceived no party differences on the other two would have been in the "Low Dem" category, and so forth. (See the Table 5 footnote for a further explanation of the "Strength of Issue Partisanship" measure.)

Now, when we have a measure which allows us to determine *degrees* of partisanship, why reduce that measure to a dichotomy; why truncate all the power out of it? And then, having taken away its power, "prove" that it is no longer "impressive?" If you lump people who are barely affected by issues with those who are highly affected, as Leege did, of course you are going to reduce the explanatory power of issues. To be sure, some of the cells in Table 6 have small N's, but that is a sampling problem, not an "interpretation" problem.

The most serious methodological question—that having to do with the desire to reduce Table 6 into "switchers" and "standpatters"—can, of course, be traced to *The Responsible Electorate*. Key left us with a very unfortunate legacy when he conceptualized the electorate as globs of "switchers" and "standpatters" (as well as "new voters"). Not only are these concepts difficult, nay, impossible, to operationalize, but they also lead to very awkward methods of analyzing the electorate.

Recall that Key defined "switchers" as those who report voting for different candidates in two succeeding elections. If one tries to operationalize "switchers" in this fashion, one comes up with a hopelessly contaminated category. There is no way to tell whether the respondents in this category defected in the *previous* election and are now simply returning to their usual voting habits or whether their *current* vote is a change from previous habits. Key assumed that the *current* vote of "switchers" was the purposeful change, although the current vote could just as likely have been rather purposeless, or simply a return to the fold.

If party identification is used as a *base* from which to measure change (as Leege did), we are on firmer ground. However, we are still in the analytically difficult position of having to analyze two types of "changers"—those who defect from party identification, and the Independents. Leege reworks my data to look at the "defectors." I thought I gave quite a bit of attention to Independents in my article, and am disappointed to hear that Leege did not feel it was enough.

But a much more fundamental shortcoming is present whenever we use categories (such as Independents, "defectors," "switchers," or whatever) as our basic units of analysis. This shortcoming has to do with the basic methodological difference between nominal and continuous measures. If we want to answer the question "How important are issues in the electoral decision?" or "How important are candidates?" or how important is any such variable, we develop ways to measure the variables—hopefully on an interval scale—and then use the appropriate quantitative method to ascertain the variables' overall degree of importance. Because I had developed interval type measures, I was able to move into a more powerful kind of analysis—multiple regression—at the end of my article. If we conceptualize the voters as categories, we are limited to nominal methods of analysis and piecemeal conclusions. Let us see how this is true in the present case.

Notice that if we use categorical analysis, about all we can say is that such-and-such a percentage of "category A" or "category B" did so and so. An example from Leege's letter is: "of those who felt an issue pull away from their own party, only 65 per cent voted for the opposition." (By the way, why "only"? Is 65 per cent high or low?) Using a continuous measure (degree of issue partisanship), I was able to say that "the stronger the issue partisanship, the greater its electoral effect . . . if several salient issues intervened, party identification had virtually no influence on voting choice." Which kind of statement goes further in explaining the importance of issues in the electoral decision? In addition, by using multiple regression analysis, I was able to measure the weight of issues in the electoral decision while controlling for both the candidate and the party identification variables. This multivariate analysis would have been extremely cumbersome, if not impossible, with nominal measures.

Finally, when Leege tries to explain the *net* effect of issues (third-from-last paragraph), he comes to a very misleading conclusion. Here he says that issues had a "net pulling power" of 6 per cent, and thus I should "scale down" my claim about the "powerful influence of issue partisanship." Spurred by his letter, I performed an analysis in the Leege fashion, using the candidate, rather than the issue, variable. In these data from the 1964 election—an election noted for the strong impact of the candidates—I found that the "net pulling power" of candidates was 9 per cent. Would Leege also conclude that candidates were of little importance in 1964? Rather than "scale down" my claims about the importance of issues, the basis for Leege's misinterpretation should be explicated.

The confusion stems from the fact that Leege failed to make certain important analytical distinctions when interpreting the data he presented in his letter; three kinds of analyses are intermixed therein. Each of these types of analysis seeks to answer a very different kind of question.

Type 1: What is the quality (rationality) of the electorate?

Type 2: How important is a certain factor (i.e., issues, candidates, etc.) in influencing voters' decisions?

Type 3: What is the *net* effect of a certain factor (i.e., issues, candidates, etc.) in the overall outcome of elections?

I have already discussed the first kind of analysis, especially the necessity of gathering a complex set of data on the quality of each voter's decision.

The second kind of analysis—determining the weight of certain factors in influencing the voters' decisions—is the kind of analysis I concentrated on performing in my article. Here the building of continuous measures (with *degrees* of influence) can lead to more definitive results, and this is what I think I succeeded in doing with my "Strength of Issue Partisanship" measure.

The third kind of analysis looks at the electorate as a whole and seeks to discover the *net* effect of a certain factor (or factors). Some researchers who are engaged in this type of analysis want to find out how much a certain factor moved the electorate to one side or the other of a 50-50 division of the vote, or what was the net effect of a factor in moving the electorate to one side or the other of the 53 per cent baseline established by Converse's "normal vote" construct. Here even a 1 per cent or 2 per cent net effect could mean the margin of victory for a party or a candidate. Thus, in this type of analysis, small figures can be very important findings.

The conclusions Leege presented in the third-from-last paragraph of his letter (where he talks about "net pulling power") are based on an analysis of this type. However, Leege was not seeking to answer the "margin of victory" question. Rather, the 6 per cent figure he obtains for the "net pulling power" of issues is based on the total defection in *both directions*—to the Democrats *and* to the Republicans. The point is, he had slipped into Type 3 analysis; he was looking at the net effect of issues across the electorate as a whole (or, in this case, among all party identifiers). One cannot expect a single factor to produce large amounts of *net* change across an entire electorate. When viewed in this perspective, the 6 per cent *net* pulling power of issues is a substantial finding.

I hope that this discussion has been helpful in clearing up some of the misinterpretations that surround this area of voting analysis.

David E. RePass
The University of Connecticut

To the Editor:

Andrew Hacker's review of *Agenda for a City: Issues Confronting New York* (*American Political Science Review,* 66 [March, 1972] 203–204), gives an inaccurate account of the contributors to that volume.

The review does not discuss the substance of the book but criticizes the fact that the contributors were "people who had ringside seats during the early 1960s." The reviewer particularly cites former members of the Wagner administration and academics with established reputations (Sayre, Netzer, Kaufman, Mansfield, and Doig) as being inappropriate choices. Eliminating those two categories would leave one with an enormously weak selection, in our judgment, but that is for the reader to decide.

The fact is, however, that Hacker avoided mention of contributors who do not suit his argument. He mentions Roger Starr, who wrote on housing, but does not mention the lead paper on that subject by Frank Kristof. Kristof's extensive analysis of the economic causes of housing abandonment and deterioration is admittedly harder to read than columns by Murray Kempton and Pete Hamill, which Hacker prefers, but people who are seriously working on solutions to housing problems find it a good deal more useful. (We share Professor Hacker's admiration for Jimmy Breslin, but he was running for high city office at the time.)

Hacker does not mention the papers by K. Dun Gifford, former legislative assistant to Senator Kennedy, and Sumner Rosen, former director of the New Careers Development Center.

We are puzzled by seeing Adam Walinsky as our "house maverick." In any case, Walinsky's contribution was co-authored by David Hertz, who was not mentioned in the review. Professor Hacker says that we ganged up on Walinsky with two commentators. He fails to mention that John Theobald (one of the two former New York City commissioners who contributed to the book) and Nathan Glazer were similarly subjected—gladly, we might add—to double commentaries. The unmentioned commentators include Charles E. Wilson, former adminstrator of the I.S. 201 demonstration school district in the early Lindsay years, and Robert A. Dentler, current director of the Center for Urban Education.

Finally, to enlighten Professor Hacker about the "fascinating" discussions as to who should contribute to the volume: (1) the Ford Foundation had nothing whatever to do with selection; (2) our major criteria were expertise for in-depth analysis of policy problems and range of opinion; (3) several former members of the Lindsay administration and leaders of community action groups were invited to participate but were unable to accept or did not complete their contributions.

We do not see the relevance to this endeavor of "janitors and cleaning women," who are certainly an unlikely source of "mavericks."

LYLE C. FITCH

Institute of Public Administration, N.Y.

TO THE EDITOR:

Mr. Fitch misread my remarks. I cannot imagine myself characterizing any discussions held under IPA auspices as "fascinating." On the contrary, I commented that "it would have been fascinating to have sat in on the discussions . . . ," alluding to the eavesdropping experience itself and not the substance overheard. But one last query. Have Mr. Fitch's interchanges with his janitors and cleaning women been so sustained as to convince him that their ideas do not deserve dissemination?

ANDREW HACKER

Queens College

EDITORIAL COMMENT

An International Journal. In American movies of a certain genre during World War II exquisite care was always taken to call attention to the wide diversity of national origins among the occupants of the bombers, infantry companies, and PT boats that we hurled against the enemy. The aim, obviously, was to make a point about the special sweetness of cooperation in a common aim among persons of heterogeneous backgrounds. Those of us who run the *Review* feel much the same as the Warner brothers on this point, although we get to express these feelings on a somewhat lower budget. We operate, also, under additional constraints. We cannot simply assign various foreign-sounding names to our authors, nor can we depart from standards of excellence and merit, applied as evenhandedly as we humanly can manage, and without regard to persons, in deciding what we publish.

Thus it is a matter of some satisfaction to us that the *Review* circulates worldwide, and that manuscripts come to us with regularity from far corners of the earth. In the last few months we have accepted articles from Kampala, Uganda; Leiden, The Netherlands; Montreal, Canada; Melbourne, Australia; Colchester, England; Jerusalem, Israel; and Los Angeles, California. There is something pleasant about the idea that we are able to sustain at least a few scholarly conversations over many thousands of miles, that our colleagues in Cambridge, Massachusetts, for example, can speak their public-regarding thoughts to political scientists on five continents, as well as to Cabots and Lowells and God.

Toward Ethical Reviewing. People may wonder if there are any rules that govern the workings of the Book Review section. Sharp-eyed observation of our own behavior these last few months suggests that in fact there are. For example, most of us know that scholars have an obligation to document assertions with accurate and responsible citations; to decline an invitation to review the work of a colleague, roommate, relative or collaborator; to refrain from reviewing a book one has commented on earlier elsewhere. These self-restraining ordinances are so thoroughly internalized throughout the profession that they need no further mention. At least, that is the conclusion warranted by 99 per cent of the transactions involved in producing the Book Review section during the last six issues of the *Review*.

We turn to that remarkable one per cent in which there were charges by an aggrieved author or reviewer of scholarly malpractice traceable to ideological bias. It is remarkable because it represents such a tiny portion of the whole, even given the profound professional differences within the discipline and those that often separate us in our lives as citizens. We exempt from this category, of course, those differences of interpretation, emphasis, and judgment that are inevitable when scholars evaluate one another's work. Such differences have led to a handful of useful exchanges in the Communications section of the *Review;* they have clarified disagreements or joined some issue more effectively.

But what about more serious problems? It would be miraculous indeed if someone were to devise an infallible means to discover and blow the whistle on transgressions against scholarship motivated consciously or unconsciously by bias. As most of us know, this simply isn't possible. The best a conscientious but fallible editor can do is to keep two caveats in mind. He must read each manuscript remembering that the reviewer has a right to state his case within the confines of scholarly discourse and civility. But an editor also needs to protect the author of a book under review from an unwarranted or prejudiced judgment. And there's the rub. While it may be somewhat easier for an editor to dispense justice than it was for Solomon to divide the baby, it turns out that in some cases there is a problem almost as bewildering of choosing between contending claims. The line between prejudiced judgment and responsible judgment may be as elusive as the missing link.

And there are practical complications. For one thing the sheer number of books received and reviewed makes it impossible for us to check systematically every suspect assertion made by a reviewer. Even if the book were still available in our office, we simply don't have the staff for such monitoring, nor could any single editor know enough to cover all cases. Moreover we do not have the warrant for such monitoring, since in dealing with our professional peers we assume their autonomy, responsibility, and accountability.

We are not in any way committed to producing a journal reflecting some ideological orthodoxy. Still, if the editor doesn't have some responsibility in trying to assure that the game is fairly played, who does?

So here is what we do. When a review is discovered to contain ad hominem remarks or what we think is ferociously aggressive editorializing, or when a review neglects the book in order to moralize about this or that fatal flaw in the American or the Uzbekistan national conscience, we talk over the problem with the reviewer hoping to achieve some agreement about the need for change. If that fails, we fall back on the principle of peer-group judgment. At least two specialists are asked to judge the merits of the review from the point of view of its propriety, scholarship, and fairness. They recommend its rejection or publication. They read the review anonymously; in this respect the same procedures are followed as those used to referee articles. The comments of the referees are passed on to the author without breaching the confidentiality that the arrangement requires from all parties. In addition, after a review reaches print, an aggrieved author still may submit a letter to the Managing Editor for publication and thus make a claim for redress before all readers of the journal. As our readers are no doubt aware, some of them do.

Errata In "To Nuture a Discipline" by Robert E. Lane in the March 1972 issue, footnote 13 (p. 167) should read "Unpublished report sent to me (8/6/'71) by John Orbell, Alvin Mushkatel, and Lawrence C. Pierce, Department of Political Science, University of Oregon."

Articles Accepted for Future Publication

Paul Abramson, Michigan State University, "Intergenerational Social Mobility and Partisan Choice"

Herbert B. Asher, Ohio State University, "The Learning of Legislative Norms"

Louis P. Benson, Kent State University, "A Research Note on Machine Politics as a Model for Change in a Philippine Province"

David W. Brady, Kansas State University, "A Research Note on the Impact of Inter-Party Competition on Congressional Voting in a Competitive Era"

Michael Brecher, Hebrew University of Jerusalem, "Images, Process, and Feedback in Foreign Policy: Israel's Decisions on German Reparations"

Eric C. Browne, University of Georgia and Mark N. Franklin, University of Strathclyde, "The Perquisites of Government: Aspects of Coalition Payoffs in European Parliamentary Democracies"

Charles S. Bullock, III, University of Georgia, "House Careerists: Changing Patterns of Longevity and Attrition"

Peter K. Eisinger, University of Wisconsin, Madison, "The Conditions of Protest Behavior in American Cities"

Robert S. Erikson, Florida State University, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections"

John Ferejohn, California Institute of Technology and Talbot Page, Resources for the Future, "A Note on 'Voting or a Price System in a Competitive Market Structure' "

Bernard Grofman and Edward N. Muller, State University of New York at Stony Brook, "The Strange Case of Relative Gratification and Potential for Political Violence: The V-Curve Hypothesis"

Richard S. Katz, Yale University, "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique"

Fred Kort, University of Connecticut, "A Theoretical Relationship for the Application of Multiple Regression Analysis to Discriminant Analysis"

Carl H. Landé, University of Kansas, "Networks and Groups in Southeast Asia: Some Observations on the Group Theory of Politics"

Wallace Mendelson, University of Texas, "From Warren to Burger: The Rise and Decline of Substantive Equal Protection"

Richard M. Merelman, University of Wisconsin, Madison, "The Structure of Policy Thinking in Adolescence: A Research Note"

Lawrence B. Mohr, University of Michigan, "The Concept of Organizational Goal"

Sarah McCally Morehouse, Manhattanville College, "The State Political Party and the Policy-Making Process"

Walter Odajnyk, Columbia University, "The Political Ideas of C. G. Jung"

Bradley M. Richardson, Ohio State University, "Urbanization and Political Behavior: The Case of Japan"

Bruce W. Robeck, Texas A&M University, "Legislative Partisanship, Constituency and Malapportionment"

Thomas W. Robinson, Council on Foreign Relations, "The Sino-Soviet Border Dispute: Background, Development, and the March 1969 Clashes"

Howard Rosenthal, Carnegie-Mellon University and Subrata Sen, University of Chicago, "Electoral Participation in the French Fifth Republic"

Lester M. Salamon, Vanderbilt University and Stephen Van Evera, University of California, Berkeley, "Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation"

Lawrence A. Scaff, University of Arizona, "Max Weber's Politics and Political Education"

Donald D. Searing, Joel J. Schwartz and Alden E. Lind, University of North Carolina, "Political Socialization and Political Belief Systems: An Essay on the Theoretical Relevance of Some Current Research"

W. Phillips Shively, University of Minnesota, "Voting Stability and the Nature of Party Attachments in the Weimar Republic"

A. H. Somjee, Simon Fraser University, "Caste and the Decline of Political Homogeneity"

Philip D. Stewart, Robert L. Arnett, William Ebert, Raymond E. McPhail, Terrence L. Rich and Craig E. Schopmeyer, Ohio State University, "Political Mobility and the Soviet Political Process: A Partial Test of Two Models"

Michael W. Suleiman, Kansas State University, "Arab Elite and Palestine-Israel"

John L. Sullivan, Iowa State University and Robert E. O'Connor, Pennsylvania State University, "Electoral Choice and Popular Control of Public Policy: The Case of the 1966 House Elections"

John L. Sullivan, Iowa State University, "A Note on Redistributive Politics"

Peter J. Taylor, University of Newcastle upon Tyne, "A New Shape Measure for Evaluating Electoral District Patterns"

Edward R. Tufte, Princeton University, "The Relationship Between Seats and Votes in Two Party Systems"

Herbert F. Weisberg, University of Michigan, "Scaling Models for Legislative Roll-Call Analysis"

Eugene R. Wittkopf, University of Florida, "Foreign Aid and United Nations Votes: A Comparative Study of Aid Allocations and Voting Agreements"

Donald A. Wittman, University of California, Santa Cruz, "Parties as Utility Maximizers"

Radical Economics: A Review Essay[*]

ROBERT L. HEILBRONER

New School for Social Research

This lucid analysis of contemporary radical thought is an effort to present both a sympathetic portrayal and a reasoned criticism of the doctrines of the New Left. As Paul Samuelson points out in his admiring introduction, Professor Lindbeck has a special qualification for this task that would be difficult to find in an American student or professor, embroiled as partisan or embattled as foe, in the currents of New Left political economics. This special qualification is that of being a foreigner, an outsider. Like Myrdal or de Tocqueville, Lindbeck approaches the problem from a "neutral" position, *au delà de la mêlée.*

What Samuelson fails to note, however, is that Lindbeck is not *au dessus de la mêlée,* for the ideas and criticisms of radical economics are an international rather than a strictly American phenomenon. However benign and impartial Lindbeck may have felt during his visits to M.I.T., Columbia, and Berkeley during the troubled year 1968–69, he brings to his analysis the preconceptions and premises of a Swedish social democrat whose economic philosophy is under the same assault in his own country as it is here. Perhaps that is why Lindbeck has produced a critique that, for all its sympathetic ear and patient tone, seems to me somehow to miss the point of much of the New Left's attack, and therefore to answer it in a way that the proponents of radical views will not find convincing.

Lindbeck sets himself two principal tasks: first, to distill the essence of the New Left arguments against contemporary neoclassical (the truer word would be nonclassical) economics; and second, to formalize the charges brought by the New Left not against economics as such, but against the economic system itself. Lindbeck sifts five basic arguments against neoclassical economics from the writings of the left.[1] They are:

1. Conventional economics tends to underestimate the importance of the question of income distribution—the pivotal issue for Ricardo, Mill, and Marx. Furthermore, when the question *is* studied, neoclassical analysis interests itself only in marginal issues, such as wage differentials based on skill, education, etc, while sidestepping the fundamental question of the division of income among *classes.*
2. Neoclassical economics develops a detailed analysis of resource allocation in response to consumer preferences, but takes the preferences of consumers as "given" rather than as a matter for investigation and explanation.
3. Conventional economics is absorbed by quantitative problems to the exclusion of qualitative ones. (I recently heard a New Left quip that sums up this criticism in a nutshell: "Capitalism is a system that transforms quality into quantity.")
4. The neoclassicists study changes at the margin but not changes in the totality. They consider economics to be the study of the variables of a given system, not of its parameters.
5. Conventional economics overlooks the interaction of economic and political processes.

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[*] Assar Lindbeck, *The Political Economy of the New Left: An Outsider's View,* with an introduction by Paul A. Samuelson (New York: Harper and Row, 1971. Pp. 100. $2.95, paper.)

[1] It is perhaps invidious to single out individual works from the monographs, books, pamphlets, and articles that constitute the literature in which Lindbeck is interested, particularly since Lindbeck's book is anything but an *ad hominem* attack. Nonetheless, it may help the reader orient himself if I identify a few books and individuals mentioned by Lindbeck as constituting spokesmen (not *the* spokesmen) for the doctrines he has set out to analyze. There is, of course, a central group whose ideas derive essentially from Marxian writing or philosophy: here we find the well-known works of Baran and Sweezy (especially *Monopoly Capital*), or Herbert Marcuse (*One Dimensional Man*). But the group is by no means exclusively Marxist. It includes rightwing anarchists such as Angus Black (*A Radical's Guide to Economic Reality*), and liberal disestablishmentarians such as J. K. Galbraith (*The New Industrial State*). Not mentioned by Lindbeck, but extremely important as current contributors to New Left thought are a group of well-trained economists out of Harvard and MIT and other bastions of respectability. Many of them belong to the Union for Radical Political Economics (URPE), and their writings will be found in the publications of that organization, in periodicals such as the *Monthly Review, Ramparts,* and other journals of the left; in various "readers," often of an eclectic cast; and occasionally in the hallowed pages of the sacred journals of the profession itself. Merely as a suggestive sampling let me mention: *Economics, Mainstream Readings and Radical Critiques* (ed. David Mermelstein); *Radical Political Economy* by Howard Sherman; *Up Against the American Myth,* written and edited by Tom Christoffel, David Finkelhor and Dan Gilbarg; *Problems in Political Economy: an Urban Perspective,* ed. David Gordon. This listing ignores numerous important, often somewhat technical papers by Herbert Gintis, Arthur MacEwan, Tom Weisskopf, and Stephen Marglin of Harvard; by Edward J. Nell and Stephen Hymer of the New School; by Michael Zweig of Stony Brook; John Gurley of Stanford; and, as the saying goes, much much more.

> Indeed, its chief difficulty is that it prides itself on being "economics" and not "political economy," thereby implying that it deals with relationships that are in some sense transhistoric. (Probably no remark so raises the hackles of the radical as the statement that economics can be defined as "the theory of choice.")

Lindbeck finds a grain of truth in each of these charges, and sometimes more than a grain. Conventional economics, he admits, *is* often small-scale in its focus and concerns; excessively technicized; indifferent to or afraid of questions involving social structure; and blind to social malfunction until that malfunction has been discovered by someone else—instances being the "discovery" of poverty by Michael Harrington, of imperialism by Magdoff and other nonconventional writers, of ecology by the Sierra Club, of the military economy by Seymour Melman, of corporate and regulatory misperformance by Ralph Nader. Hence his tone is conciliatory. But his judgment stops short of endorsing the radical attack on neoclassical economics as a sterile or apologistic pursuit. Rather, Lindbeck seeks to demonstrate that neoclassical economics in fact possesses the "tools" to investigate and clarify these problems, and that its failures must therefore be chalked up to whatever social processes or pressures have diverted the main body of the profession away from serious matters, rather than to any fundamental shortcomings of the discipline itself.

Is this defense well taken? I do not think that Lindbeck makes as strong a case for his contention as he believes. With regard to the question of income distribution, for example, he cites as support for the capabilities of neoclassical economics recent studies by Gary Becker and Jacob Mincer, although this is precisely the sort of work attacked by the New Left because it begins from assumptions whose legitimacy they question. In particular, the radical economists attack the validity of two key concepts in all neoclassical explanations of income distribution, asking whether "marginal productivity" in fact measures anything that can (or should) be used as a criterion for analyzing income shares, and disputing the much-touted idea of "human capital" because it swallows whole the very concept—capital—that needs to be criticized.

Lindbeck is more favorably inclined toward argument (2)—the neoclassical assumption that tastes are autonomous—and merely cautions against naive generalizations about the ease with which tastes may be formed or manipulated. This caution is fully justified. But Lindbeck fails to respond to the underlying thrust of the New Left critique—that *all* tastes in a society dominated by the imperatives of a commercial civilization are in some deep manner suspect. Although few radicals claim to know what the "natural" tastes of men are or should be, they call into question the basic acquiesence of conventional economics before the conception of a "utility-maximizing" world, and its indifference to the socioeconomic preconditions for such a world.

The quality of life—criticism (3)—is, as Lindbeck says, a problem to which conventional economics has until recently paid little heed. His reply to the New Left criticism is that there is now an "interesting and growing" literature" in which various methods of dealing with externalities are examined. Here Lindbeck misses two points. A large portion of the New Left critique with regard to the quality of life has to do with the alienation of men under conditions of industrial labor and intensive consumer pressure: Lindbeck does not acknowledge the virtual absence of any matching concern within conventional economics. Further, with respect to the problem of externalities proper, the New Left is interested not merely in the existence of externalities or the presence of economic tools, such as taxes, regulation, or subsidies, to minimize them. It is interested in detecting within the class of problems having to do with externalities that subset whose roots lie in the specific drives and constraints of capitalism. It is no doubt true that the New Left often confuses the general problems of industrial pollution with the special problems of capitalist pollution; but it is also true that conventional economics pays little or no heed to special kinds of environmental damage that are directly traceable to the specific forms of production dictated by capitalist institutions.

I shall bypass Lindbeck's treatment of the fourth point—he grants that economists have not been much interested in large-scale historical processes or in comparative economic systems—and focus on the last question: the problem of economics versus political economy.

Here Lindbeck again seems to miss the meaning of the New Left criticism. He admits that economists have not paid much attention to such problems as lobbying or the means by which special groups win special privileges. He grants as well that little attention has been paid to the interaction between domestic interests and foreign policy—in short, imperialism. His countercriticism is that the methods used by the New Left to examine these questions are inadequate—a mixture of vulgar Marxism, mechanical linkages of the "interests" of big business and the state, etc. What is needed, he suggests, is empirical work that will cut through these theoretical misconceptions to study the precise means "by which various domestic pressure groups obtain privileges through economic and other types of legislation and of how foreign domination may sometimes occur through foreign investments, foreign aid, trade policies, military policies, and the like."

Who can fault such a prescription? The problem is, can it be filled by the present ideas of neoclassical economics? Can conventional economists explain the existence or the stubborn persistence of the upper tail of the Lorenz curve of income distribution, despite "progressive" taxation? Can it explain why international trade, the theory of which promises that exchange will yield benefits to both parties, has resulted in the polarization of a highly developed and a highly underdeveloped world? Can it account for the persistence of poverty despite the supposedly curative effects of growth? I fear the answer to these questions is No. I would agree with Lindbeck that the New Left often explains these and other aspects of socio-economic reality in ways that strike me as tendentious or simply wrong. But at least the problems are faced. They are not faced by the majority of conventional economists. Moreover there is little evidence that the "tools" of conventional theory will unravel problems whose difficulty is that they are not problems of "economics" but of political economy.

The second section of Lindbeck's book, in which he deals with the New Left's criticism of the economy, is stronger and more thought-provoking than the first. Rather than enumerate Lindbeck's summary of the New Left's objections to the economic system, let me concentrate on one issue that repeatedly surfaces in this portion of the book. This is a pointed emphasis on a "contradiction" in which the New Left finds itself by virtue of its opposition to two aspects of contemporary society. As Lindbeck puts it:

> One characteristic feature of the New Left movement is that most of its adherents are strongly opposed to markets. In the literature of the New Left, a market system is denounced as primitive, inefficient, chaotic, antisocial, unfair, and basically immoral.
>
> One problem with this position is that most of the New Left's writers in this field are also strongly opposed to bureaucracy, that is, to formalized hierarchical administration. For instance, the bureaucracy in the Soviet Union is often criticized in New Left literature. . . . It may be possible to make a strong case against either markets or administrative systems, but if we are against *both* we are in trouble; there is hardly a third method for allocating resources and coordinating economic decisions, if we eliminate physical force. Both markets and administrative procedures may, of course, take many different forms. Markets may be more or less competitive and administrative procedures more or less centralized, with some decision-making by vote, and so on. *Thus. the more strongly we are against bureaucracy, the more we should be in favor of markets.* (pp. 32–33, italics in text).

Lindbeck's thrust is a shrewd one, but I am made somewhat uneasy by the use he makes of it. For one thing, he is not technically correct in asserting that market/bureaucracy is an either/or proposition. The mode of production and distribution in primitive societies is regulated by an internalized self-discipline called *tradition;* and it is entirely possible that some form of communitarian socialism might rely on the same mechanism in the future. Indeed, as Samuelson says in his introduction, the "utopian self-sufficient kibbutz" *is* an alternative to both market and bureaucracy.

Then, too, I am unhappy at Lindbeck's failure to take seriously his own qualifying phrase that "markets may be more or less competitive and administrative procedures more or less centralized" (p. 33). Throughout the remainder of the text we are faced with the dilemma of market or bureaucracy without the shadings that may make this choice in fact a good deal less decisive or contradictory than Lindbeck implies. For example, Lindbeck does not differentiate between the market for *goods,* which many New Left critics would accept, at least for a provisional period, and that for labor, which most (but not all) radicals would not accept, certainly under the ground rules of property rights. In addition, Lindbeck does not raise for our consideration the possibilities of tempering bureaucracy through reforms that go in the Yugoslav or Chinese directions. Although we have almost no detailed knowledge of the effectiveness of these efforts, there is no doubt that both nations are trying to avoid the monolith of Sovietism that Lindbeck unconsciously makes the sole paradigm of bureaucratic allocation. Thus we are faced with a false choice in his iron dichotomy, and his clear preference for a market solution results more from his opposition to an oversimplified ideal type of bureaucracy than from a careful discussion of possible alternatives.

It must be clear that I have serious doubts about the effectiveness of Lindbeck's counter-arguments to the New Left. Yet I would be doing his book a considerable injustice if I were to leave the reader with an impression only of its weaknesses. Terse and often penetrating, Lindbeck's essay reminds me in its simplicity and clarification of Robert Dahl's *After the Revolution?* In both books, the puzzles and difficulties that slogans hide from us are ruthlessly dragged out for examination. I can only applaud when Lindbeck stresses the blurred correlation between the structure of ownership on the one hand and political and social conditions on the other (Sweden and the Union of South Africa both rest atop a base of private property and market relations). I am impressed by his discussion of the costs of obtaining information in a marketless society and with the social results of failing to do so. I am struck (as is Samuelson) by his telling critique of Galbraith's theory of demand-creation.

Most important of all is Lindbeck's function in pointing out to the New Left the tasks to which it must sooner or later address itself. Judging by the radical literature with which I am familiar, the New Left is almost entirely concerned with criticism, not program. Yet, as Lindbeck points out:

> As all serious scholars know, the real problems start *after* the revolution or, what from this point of view is about the same thing, *without* a revolution. However there is very little, if any, discussion in the literature of the New Left about the *methods* of solving the problems which chiefly worry economists, such as the reconciliation of full employment with price stability and equilibrium in the balance of payments; the determination of an optimal growth rate and hence an optimal combination of consumption tomorrow . . . ; the design of a workable compromise between the income differentials needed to give incentives for work and for the allocation of the labor force, and the desire for equality; . . . whether employees in the public sector should have the right to strike; . . . how to avoid an enormous concentration of power in a small group of politicians and administrators in a society with growing government intervention; . . . the advantages and disadvantages of multinational firms; whether important decisions of social and economic policy can really be efficiently carried out by the national state. . . . On most of these difficult and important problems the New Left is quite silent or superficial (pp. 98–99).

The indictment is a powerful one—until one asks whether conventional economics is not also silent or superficial before most of these issues. Thus once again, Lindbeck's book is not quite the "outsider's" view that it claims to be. The danger is therefore that its clear and calm exposition will be taken by the neoclassical economist as a definitive answer to questions and problems to which it does not, in fact, do full justice. Nonetheless, Lindbeck could perform an invaluable service if he would motivate members of the New Left to address themselves to the sorts of issues he raises in the paragraph quoted above. I rather suspect, however, that if these problems are to be examined with the care they deserve, it will not be until they are raised with the same degree of clarity and urgency by someone who is an insider unwilling to postpone the agenda of the future until the revolution brings it home with stunning force.

BOOK REVIEWS

Hegel's Political Philosophy: Problems and Perspectives. Ed. Z. A. Pelczynski (Cambridge: University Press, 1971. Pp. 246. $14.50.)

This collection differs from some other recent collections of articles on Hegel by consisting exclusively of essays that have not appeared previously. *New Studies in Hegel's Philosophy* (1971), ed. Warren E. Steinkraus, offers fourteen studies, some old, some new; *Hegel's Political Philosophy* (1970), ed. Walter Kaufmann, ten, all of them published previously. The price one has to pay for insisting on contributions that have not appeared in print before is, of course, that some of the best known Hegel scholars are not represented because they lacked the time to write new pieces especially for this volume. Nevertheless, the cast is impressive. Apart from the editor who is the only writer represented by two essays (the first and the last), there are John Plamenatz (Oxford), W. H. Walsh (Edinburgh), and R. N. Berki (Hull) from the United Kingdom; J.-F. Suter and Eugène Fleischmann from Paris; K.-H. Ilting (Saar) and Manfred Riedel (Erlangen) from Germany G. Heiman (Toronto); and Judith N. Shklar (Harvard), David E. Cooper (University of Miami), and D. P. Verene (Pennsylvania State University) from the U.S.

The level of scholarship throughout this volume is imposing. Nobody wrote his contribution "off the top of his head." The average number of footnotes per essay is more than sixty; most of the references are to Hegel's writings; and all of the contributors seem to feel very much at home with Hegel. The other side of that coin is that the volume makes pretty hard reading. It is not to be expected that many scholars, not to speak of others, will read the whole book from beginning to end. For that reason, the printing is small and the price staggering ($14.50 for less than 250 pages, including a good index). The price will do its share to limit the audience.

In general, one may well wonder about the point of such books. The chief value of the present volume may be that it sets high standards of scholarship in a field in which shoddy scholarship has been all too widespread. Those who merely read some of the essays may still be immunized against the facile generalizations about Hegel's political philosophy that were considered respectable until quite recently.

It is nevertheless a pity that most of these articles are not more readable. The fault is neither Hegel's nor the editor's. Quotations from Hegel do not stand out as curiosities in generally lucid prose, and the difficulty does not lie in sentences that have to be reread again and again. Instead most of the prose is simply drab, and there is rarely any sense of intellectual excitement. Anyone looking for crisply stated theses that are at variance with common "knowledge" but defended with at least a little virtuosity will certainly be disappointed by much of this volume.

Occasionally, the editor interposes brief footnotes calling attention to the fact that another contributor deals with the same problem on another page: thus Fleischmann says in passing that Hegel admired Antigone as the representative of individual conscience and conviction, while Shklar argues at some length that he did not see her that way and admired her for different reasons:

> She never thought of herself as an individual expressing a personal morality. She spoke solely as a sister within an ethical family, as a social being, fulfilling a defined role. . . . She did not look into her heart or conscience to discover righteousness. She knew what had to be done, and always had known, because she was not making a moral choice, but obeying an unquestionable law. (p. 85).

The issue is never joined, and the title of Shklar's essay is "Hegel's '*Phenomenology*': An elegy for Hellas." Here, too, as in most of the essays, a theme is pursued, and it is not a matter of establishing a new view. E. M. Butler's bold book, *The Tyranny of Greece over Germany* (1935), was much more exciting. Not only was her theme newer then, but she rapidly covered a great deal of territory and showed us all of it in a willful but clearly defined and revelatory perspective.

The new book is a miscellany of scholarly articles. They deal with such topics as Hegel's conception of the state, his conception of history as the realization of freedom, the structure of his *Philosophy of Right*, his "corporate doctrine," his theory of punishment, and his account of war. Three pieces involve comparisons with other thinkers: "Burke, Hegel, and the French Revolution," "Perspectives in the Marxian critique of Hegel's political philosophy," and a very short article on Stirner, Marx, and Hegel. Anyone dealing with one of these topics would be well advised to consult the relevant essay. Anyone not dealing with one of these topics in the first place is not very likely to become excited about them by reading these essays. Nevertheless this volume evokes not only respect but gratitude. It may well represent a milestone in the study of Hegel's political philosophy and the end of an era of irresponsibility. The picture of Hegel that emerges here bears little resemblance to the bogeyman presented to us by his detractors. Nor does the book revive the Hegel image of the British Idealists. The new portrait is much more accurate. But it does not capture the distinctive and often bizarre boldness of this unique thinker.

WALTER KAUFMANN

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From Karl Mannheim. Edited and with an Introduction by Kurt H. Wolff. (New York: Oxford University Press, 1971. Pp. cxi, 393. $12.50.)

In the 1920s Karl Mannheim's *Ideology and Utopia* created quite a stir among German intellectuals by showing that not only *what* men think politically but also *how* they think is conditioned by their social position. Mannheim developed a method of "sociology of knowledge" which enabled him to demonstrate the dependence of the *structure* of thought on the social position of the thinker. This, of course, was a refinement and at the same time a subtle overcoming of Marxist doctrine. Mannheim was one of the few sociologists who in Weimar Germany was able to develop a "school." One of his last efforts was an attempt to gain official recognition for sociology as a "teachable" discipline.

After his flight from the Nazi Regime, Mannheim contributed to the development of sociology as a field of university instruction in England, where it had hardly gained a foothold at the London School of Economics and none at Oxford and Cambridge. The present selection from his writings covers both periods. The essays have been available in English before, except one which is given in Wolff's translation (Problems of Sociology in Germany).

Kurt H. Wolff, already known as editor and interpreter of Georg Simmel's works gives in his Introduction of more than one hundred thirty pages a veritable "course" on Mannheim in which he explains the origin, intention, and significance of each of Mannheim's major writings, including many not contained in this selection. The well-chosen readings will give the student an idea of Mannheim's work, which is the main purpose of a collection of this type.

The Introduction is the really new feature of the book. The author discusses Mannheim's major writings in chronological order, inserting in some instances long passages from papers not included in the Reader. The arrangement may seem rather dull at first glance, but it is not, for Wolff uses it to demonstrate the development of Mannheim's thinking. Like many of the earlier great sociologists, Mannheim started out not as a sociologist but as a philosopher with predominant interest in epistemology. When, after his migration from Hungary to Germany, he turned to sociology, it was the recently opened field of sociology of knowledge which he mainly cultivated and in which he gained an international reputation. His contributions were by no means restricted to methodological discourses but included empirical inquiries as well. Of these the essays on "Conservative Thought" and on "Democratization of Culture" are likely to be of major interest to political scientists, as would also be the essay on "The Problem of Generations" which is *not* included in the readings but is summarized in the Introduction.

After his flight to England, Mannheim changed his concerns, as is well known among students of his later work. More than ever before opposed to any kind of totalitarian regime, he became increasingly concerned with the possibilities (and necessity) of planning in a democratic society, the problem being the preservation of freedom and human dignity in a planned economy.

Wolff's Introduction is a profound, empathic and at the same time critical piece of what may be called intellectual biography. It covers more ground and goes deeper into the matter than Paul Kecskemeti's short but competent introduction to the British publication "Essays on Sociology and Social Psychology" of 1953. It must be said that Wolff does not make it easy, at least not for readers who are unfamiliar with the problems of epistemology and sociology of knowledge. One is tempted to say, what is called "introduction" is rather an analytical, interpretive, and critical essay to be read after the "readings," or at least as an accompaniment. Finally, one wishes that instead of keeping the discourse mainly within the framework set by Mannheim's work, the author had said more about the intellectual and academic milieu in which Mannheim's ideas developed. In that way such seemingly "dry" and "dated" pieces as the lecture on the "Tasks of Sociology" (p. lxxvi ff.) would gain life. For this reviewer, who was present when it was delivered, it was a great and unforgettable experience.

RUDOLF HEBERLE

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Critique of Hegel's Philosophy of Right. By Karl Marx. Edited by Joseph O'Malley (Cambridge: Cambridge Univ. Press, 1970. Pp. 219. $10.50.)

This is the first full English translation of Marx's commentary on sections 261–313 of Hegel's *Philosophy of Right.* In his sensitive and scholarly introduction, O'Malley aptly characterizes this unfinished manuscript of 1843 as

> Marx's first effort to expose and criticize Hegel's philosophy in general and his political philosophy in particular; and through this effort both to criticize existing political institutions and, with the help of research in political theory and history, to clarify the relation between the political and economic aspects of society.

Unfortunately, this is not only Marx's first, but his only attempt to swallow Hegel and his subject matter whole. In the next six months, Marx reorganized his plan of exposition and published a much shorter piece called "Introduction to the Critique of Hegel's *Philosophy of Right,*" which also appears in O'Malley's volume, but the series of studies of Hegel's views on politics, law, ethics, etc. which were to follow gave way to more pressing concerns. Though Hegel's general philosophy receives attention in several of Marx's writings of the '40s —most notably in the *Holy Family* and the *1844 Manuscripts*—the oft-mentioned debt to Hegel (and to future Marx-Hegel scholars) remains unpaid. Similarly, though the state is a major variable whenever the materialist conception of history appears, and although an implicit theory of the state provides the framework for several empirical studies—particularly of French politics—Marx never again examined systematically the various forms of the state in their interconnection with the other institutions and practices of capitalist society.

Marx's plans for presenting his ideas in economics and history also underwent frequent changes. Witness, for example, the different approaches to the capitalist economy found in the *Grundrisse,* the *Critique of Political Economy,* and the German and French editions of *Capital,* volume I. What chiefly distinguishes Marx's views on the state from his views on the economy is that no finished work on the former ever appeared. It is interesting to speculate what the study of Marxist economics would be like had the *Grundrisse,* Marx's disjointed and highly philosophical draft of *Capital,* contained his most systematic treatment of the subject. Yet, this is very much the position of political scientists and others wishing to understand Marx's theory of the state before his *Critique of Hegel's Philosophy of Right;* we are condemned to work out the full plot of a story from an account that offers only clues. Hence the careful attention this work deserves and will increasingly get. Hence, too, the caution with which it must be approached.

In the *Philosophy of Right,* Hegel tries to show how the modern state is able to reconcile the conflicting demands of private and public life (the needs and duties of *burgher* and *citoyen*) through the interplay of idealized versions of monarchy, bureaucracy, and parliament. For Hegel, the competition between particular interests, and with it the alienation of each from all which characterizes social existence, can be overcome by the construction of state forms that express the general interest. Marx, who agrees with and even amplifies Hegel's vision of the social order, denies the possibility of achieving political community on such a basis. His fledgling research had already convinced him that the state in its various forms and practices reflects the clash of private interests and that talk of general interests only serves to disguise the victory of the stronger party. Marx accuses Hegel essentially of accepting political alienation in the guise of resolving it.

When Marx wrote the *Critique,* many of the ideas we associate with his name, such as the labor theory of value and even the emancipatory role of the proletariat, had not been thought of. Still, the analysis of private property (probably the centerpiece of the entire work), the charting of its alienated character and political influence, is fully in accord with his later writings. In my view, the *Critique* offers us not so much an early stage in Marx's theoretical progress as a onesided version of the analysis that has come to be known as Marxism, a version that focuses on the nexus of private property and the state. In the course of this critique, Marx reconstructs the ties he sees among constitution, executive, legislature, judiciary and bureaucracy as well as between these forms of the state and the different forms of private property. Marx's well-known claim that the state is the "executive committee" of the ruling class is easily reduced to caricature if one is unfamiliar with the rich context provided by this early work.

Another important virtue of Marx's *Critique of Hegel's Philosophy of Right* lies in the insight it affords into his method, not only for treating Hegel but for investigating society at large, and here too O'Malley's Introduction enhances considerably the organization and clarity achieved in the text.

Marx said he undertook this critique of Hegel to work out the role of material factors in society and to liberate the dialectic from the mystical form with which Hegel endowed it. Readers of this difficult work will understand these central features of Marxism better for having witnessed their birth.

BERTELL OLLMAN

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The Significance Test Controversy—A Reader. Edited by Denton E. Morrison and Ramon E. Henkel. (Chicago: Aldine Publishing Company, 1970. Pp. xviii, 333. $12.50.)

Most of the thirty selections in this reader were written by social scientists for an audience of social scientists. A verbal (nonmathematical) presentation characterizes the editors' comments as well as the papers. The subject of the reader—correct, creative use of tests of significance and other modes of statistical inference—is important for the social scientist. (A useful summary is that written by William H. Kruskal, "Significance, Tests of," *International Encyclopedia of the Social Sciences,* [The Macmillan Company & The Free Press: New York, 1968], Vol. 14, 238–250.)

Part 1 of the reader consists of three chapters by Lancelot Hogben. The editors warn us "his style is abstruse," and in Chapter 3 he writes "with increasing invective" against Fisherian statistics; the editors also say "Hogben is delightful and rewarding reading."

Part 2 contains nineteen selections from sociological literature after 1941. The editors properly praise Margaret Hagood's early serious effort to interpret the mathematical model of statistics in the framework of sociological research. The negativistic attitude toward statistics of the Bureau of Applied Social Research (c. 1955) is illustrated.

Part 3 contains five articles by psychologists, part 4 gives three "criticisms from other quarters," and part 5 is the epilogue of the editors. The distinguished psychologist, Paul Meehl, tells philosophers how social scientists achieve near nonsense by blind application of hypothesis testing procedures.

The following is my view of the main thrust of the articles in this volume: Mechanical applications of statistical procedures in social science research are often used, although seldom appropriate. The relevant journal editors seem to encourage, if not insist on, routine statistical analysis. But fruitful applications of statistics require much effort. Unfortunately, most scientists have not been exposed in their academic programs or in their work experience to the fundamental aspects of a statistical problem. It is not surprising, therefore, that most scientists act as if the statistical structure of a problem should be an afterthought requiring little effort. But the statistical and probabilistic structure of many problems, particularly in the social sciences, must often be an important part of the underlying theory, must often control the choice of experiments, and must often be a major component of the analysis of the research.

Social scientists in their training programs and in their research activities have placed an unreasonable emphasis on statistical technology. They are fascinated by the formalism of analysis of variance, Fisher's exact test, etc. without being prepared to recognize or face the difficulties in the correct use of the technology. This concentration on technology has cut social scientists' resources in two directions: they have tended to emphasize the simplistic, such as hypothesis testing, in contrast to the comprehensive, such as decision theory; and also, although they may be aware that there are differences between schools of statisticians, (such as frequentists and subjectivists), social scientists have tended not to learn what the differences are, nor have they questioned which, if any, of the schools are most related to their own theorizing.

These shortcomings can be partially explained by the time lag required for developments in one field (statistics) to become useful in other fields (social sciences). Also, the difficulty of some of the statistical concepts insures a slow learning rate. Nevertheless, in large part the social sciences have fallen into an irrational position: social science is committed to a large statistical component, and this effort is kept at a low intellectual level, static in content and application.

It seems to me that the controversies in the social science literature regarding statistics are of limited interest. They frequently arise from polemical writing, characterized by wooly verbal arguments instead of available precise mathematical formulations. "Controversies" such as one-sided versus two-sided tests of significance, statistical significance versus importance can be clarified with the help of precise definitions and notations. If there is a controversy it relates to basic issues—frequentists versus subjectivists—and not to technicalities.

On a more specific level, the thirty selections cover about twenty years and with one exception were all published in standard American places. Thus, it is possible to follow the citations of a few papers, but the historical view is restricted. The editors have added useful summarizing and analytical chapters about the volume's articles. Several enlightening footnotes and a unified list of references are presented.

Although the volume is well printed, one feels a bit disappointed in not having the original articles available for checking. Missing are the professional affiliations of the authors and the asterisk footnotes at the beginning of articles, which contain the history of each article. At least one abstract is also missing. Footnote numbering never corresponds to the original and footnote wording often contains variation from the original. One misses having the references with the article; there is more than one error in the indication of the origins of the readings.

In summary, Morrison and Henkel present the social scientists' view of what they consider to be major statistical issues. Their material strongly supports the proposition that it is difficult to make good use of statistics, and that routine use of statistical procedures courts disaster.

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Between Nothingness and Paradise. By Gerhart Niemeyer. (Baton Rouge: Louisiana State University Press, 1971. Pp. ix, 226. $8.95.)

This slim, attractively printed volume consists of seven chapters most of which could easily stand as individual essays. In fact, two had been previously published elsewhere. The author's aims, as laid out in the preface, are both explanatory and normative. He seeks to clarify the total critique of society and the concomitant "creative destruction" which he identifies as features common to all totalitarian ideologies. Beyond this, however, he looks for the justifiable limits of social criticism. "On what grounds," he asks, "can one arrive at something like a norm regarding man's proper attitude toward society's existence?"

Although Niemeyer writes well and has some interesting things to say, the book falls considerably short of the expectations generated by the preface and particularly by the publisher's book-jacket which allude to new insights on totalitarianism, Nazism, and the New Left, among several other items. Actually, the first chapter discusses the ideas of eighteenth-century French ideologues—Meslier, Morelly, Mably, and Babeuf. The second deals with Turgot, Condorcet, and Fourier. In the third chapter the so-called axiological and teleological critiques of society thus far illustrated are brought together, principally in the discussion of Marx, who is said to have "combine [d] these two in one system." (p. 79). Chapter 4, titled "Totalitarian Activism," focuses on the political ideas of Lenin and it rounds out that part of the book which is supposedly devoted to an analysis of totalitarianism. For Niemeyer totalitarians are those persons for whom "society represents evil in general and their existential rejection of it engulfs ethics and metaphysics, politics and religion" (p. vii).

Throughout, Niemeyer advances a sensible enough view that those who criticize existent societies in a root-and-branch fashion are apt to negate man himself, at least man in all his historical, particular, social, and cultural concreteness. A sweeping rejection of all that society is, and has been, in behalf of some phantasmal "ought-to-be"—or "will-be"—may well culminate in politically "logical murder," in nihilistic destruction of all that seems imperfect or obsolescent. To be sure, he does not consider that massive destruction may also be the policy of those who profess wholehearted acceptance of a particular *status quo*—if they wish to stem heresy and rebellion desperately enough. Indeed, fanaticism may well be a more important cause of "logical murder" than either the apparent affirmation or negation of any given social order or public past.

In any case, Niemeyer's application of his own formula to particulars—both ideological and social ones—leaves much to be desired. His interpretation of Marx as a fountainhead of totalitarianism—the foremost nihilistic, total critic of society—seems greatly overdone to this reader and marred by numerous unjustified inferences. More important, it does not come to grips with those aspects of Marx's own writings and the considerable body of Marxian scholarship which contradict this image. Marx credited the bourgeoisie with a historically positive role of bringing about the indispensable economic and social conditions of a truly free, classless society that was to follow. He asserted that individual capitalists were not to blame for those evils which capitalism as an institution was perpetrating (simultaneously with its positive achievements) upon the proletariat. The specific measures that Marx advocated against capitalists in the *Manifesto* must seem extraordinarily tepid to anyone familiar with the practices of Stalinism, Maoism, or Nazism. His concern with the historic evolution of man toward ever higher plateaus of social existence is much too easily reduced by the author to a totally nihilistic rejection of everything that is and has been. The Marxian vision becomes a leap from nothing to everything.

Niemeyer designates Lenin as the one who "took over all the essential elements of Marx's world view . . . without changing them" (p. 103). The reader may well wonder why the award for orthodoxy should thus go to Lenin. In fact, the author discusses at some length (pp. 115–125) the very important debate between Karl Kautsky and Lenin on the question of democracy or dictatorship for the postrevolutionary state. In the course of this discussion, he says: "Kautsky did envisage that the Revolution would culminate in a regime founded on the community of reason and devoted to justice, in which the shared notion of the common good would unite people in trust, friendship, and peace. Lenin, addressing himself to the same problem, had the concept not of political order but of battle order, an association mainly for the elimination of enemy forces" (p. 123).

Now, if Lenin's "totalitarianism" owes its being to Marxian ideas, how do we account for Kautsky, who drank, as it were, from the same poisoned well? Clearly, even Lenin—as Niemeyer admits—had considered Kautsky for many years "the leading Marxist theoretician" (p. 116). Might the poison of Russian totalitarianism *not* be attributable to the philosophy of Marx after all? And if Marx cannot be convicted of Leninism, the totalitarian connections of Stalinism and Maoism to Hegel and to Morelly, Mably, and all the rest appear even more tenuous.

Since the author omits all substantive mention of Nazism or Fascism (except for, literally, one sentence on page vii of the preface, one on page 139, and two on page 203), the reader can merely wonder what "negations" these ideologies represent. The whole subject is simply reduced to a discussion of Lenin's Communism. The author does not take up either the problem of the implementation of ideology through political institutions or any factors which, either in conjunction with ideology or apart from it, may promote the radical rejection of society and the nihilistic destruction of lives and institutions doubtlessly evident in the twentieth century.

The concluding three chapters furnish an interesting examination of the significance of a public past, of certain shared remembrances that provide society with political linkages of mutual understanding and friendship. Without these, Niemeyer argues, communities cannot function effectively. Essentially, his conclusion is that social criticism like all "beliefs about public order must have something of the quality of a mother's belief in her wayward boy, an activity of intellect and consciousness that is capable of sustaining the given condition of belonging-to-each-other" (p. 206). The examples of the prophets of ancient Israel, of Socrates, and Augustine are all effectively used to illustrate Niemeyer's theme. If the author is to be judged mainly by the objective stated in his conclusion "to foment a process of returning awareness of what it is that feeds the widespread disorder of our age and what and where are the sources of our order," then, despite all the difficulties, his efforts have borne some fruit. The book provides stimulating reading on subjects of enduring political importance.

ALEXANDER J. GROTH

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Democracy and Revolution. By George Novak. (N.Y.: Pathfinder Press, 1971. Pp. 286. $2.95, paper.)

George Novak's publisher hails his book (on its cover) as "the first comprehensive history from the Marxist standpoint of democracy in all its important aspects." Surely, the title is one to conjure with. But Novak's magic is of an old and well-worn kind. His familiar is Trotsky, his invocation "scientific socialism," his subject the "revolutionary masses," from classical Athens to present-day America. Democracy, he tells us in his foreword, must be demystified. It has different meanings because it refers to changing "products" of socioeconomic development, and its real prospects can be identified by understanding its evolution. Indeed, "Economics is more decisive than politics in determining the essence of a regime" (p. 173); "As a rule, political developments do not keep pace with economic changes but limp on crutches behind them" (p. 149). Primitive democracy was simply collective, egalitarian society, without class or state, wherein no man was subject to external authority and decisions were made by unanimity; political democracy (both precapitalist and capitalist) is a form of state rule in class society, one under which the many theoretically have supremacy over the few but where social dictatorship nonetheless exists; both bourgeois political democracy and capitalism, which it serves, are "destined to be superseded by a higher form" (p. 13). "Democracy is a good thing. More democracy is still better" (p. 10) —and more democracy today means the replacement of bourgeois democracy by workers' social democracy, a replacement which, as with past transformations, will require revolution.

The problems here are as familiar as the spell—the characteristics of primitive society, the locus and consciousness of class, the forms and occasions of revolution, the relations between politcal and social (let alone "economic"), the place of cultural factors, symbolic meaning, everyday language, ideology. And on the level of analysis which Novak adopts, there are few intriguing theoretical subtleties—no explorations of *Praxis,* objectification, or social being, by which to grapple with the old conundrums. Throughout his general discussion of bourgeois democracy (the bulk of his book), Novak's terminology has an old-fashioned ring. It hardly seems likely to impress sophisticated Marxist scholars. For that matter, in his discussion of contemporary society, Novak makes no reference to such issues as massification, the socializing impact of multiple patterns of authority, or even managerialism; he mentions women's and third-world liberation only briefly, and only as converging auxiliaries to his projected "workers' " revolution; he contrasts bourgeois-democratic "ideology" with his own "scientific" stance, sketching the development (*à la* MacPherson) only of the former; he ignores the recent controversies over community power (he cites C. Wright Mills, neglecting to mention Mills's own non-Marxist stance). And the historical sections of Novak's book scarcely offer scope for real analysis. He gives us a few pages each on "precapitalist forms" of political democracy (Greece, Rome, the medieval communes) and on the "six decisive steps in the forward march of the bourgeois-democratic revolution" (Holland, England, America, France, 1848, the American Civil War). In no case do the notes and bibliography reveal more than the scantiest of sources. In each the guiding thesis is enunciated rather than put to work with detailed precision. No reference to conciliar theory or ecclesiastical representative practices in the Middle Ages. No coherent and consistent sketch of the socioeconomic forces which supposedly "produced" or hampered the three general cases of precapitalist democracy, nor any clear criteria for identifying the particular regime selected as politically democratic (proportion of inhabitants with citizens' rights? extent of citizens' rights? social position of citizens?). . . . But take by far the most extensive of the historical treatments, that of Greece (pp. 20–32). "Homer believed in the divine right of kings," we are told—scarcely a statement to inspire confidence in the historical or conceptual sense at work. We are also told that there was a "cycle" of kingship, aristocracy, oligarchy, tyranny, democracy, and that only wealthy trading polities completed the cycle and thus reaped the fruits of democracy, "dialectically" prepared for by its opposite, tyranny. Now the histories of the polities fail to fit so neat a sequence at all well, either individually or collectively; far from investigating the evidence, Novak neglects even Aristotle's famous critique of Plato on the point. And conceptually, at the very least, one would like to know how the supposed relation of tyranny and democracy squares with Novak's professed stress on the "economic." Equally sweeping is his summary of Athenian history—a great and popular "democratic revolution," from the early sixth century to Pericles, led by the Party of the Hill, won only by direct conflict and never by reform, resulting in the replacement of the old landowners by the new "middle classes." No reference to Solon, to the impact of Persia, to the cult of the *bios politikos* portrayed by Thucydides, to the shifting complexities of factions and constitutions.

In a sense, such detailed analysis is beside the point. Novak's is not really a work of scholarly comparison, let alone one of theoretical innovation. It is more thorough conviction than questioning exploration. It must be read as a contemporary tract. Novak's comments on earlier versions of political democracy are really prologues to his concern with the establishment of social democracy in the United States. (He dedicates his book to the shade of Tom Paine, and stresses the American revolutionary tradition throughout.) Even the section on Greece (the initial one) serves above all to introduce his belief in direct popular action, the necessity of revolution, and the links between purely political democracy and social dictatorship. The real interest of his book is how Novak links these themes with his projection of *post*-capitalist democracy. Here his Trotskyism emerges most clearly. But here, too, the analytic difficulties reappear. Novak's usage of "democracy" at times seems to resemble a very old shell-game indeed: now you see it, now you don't. (On p. 32 he notes that "social democracy" will also be a "social dictatorship," albeit one based on "non-exploitative" productive relations; on pp. 227–228, that it too will need a state, and will need to take "repressive measures" against active counter-revolutionaries; on pp. 77–78, that the "liberal" view of democracy and dictatorship as polar opposites is a shallow one—Robespierre's regime, for example, "provided the fullest measure of democracy the French people had ever enjoyed. It was a democratic dictatorship.") Still, Novak's desire to extend democracy in some unambiguous political fashion seems evident enough. He criticizes the lumping of bourgeois democracy together with such "capitalist variants" as Bonapartism, military dictatorship, and fascism: bourgeois democracy is far preferable to these, and some of its achievements (freedom of speech, judicial rights, etc.) must be safeguarded in a social democracy. He notes that "certain tendencies toward bureaucratism would necessarily manifest themselves in the passage from capitalism to socialism" (p. 235); but he sees mass political participation and strong "political leadership" as the solutions, and takes the USSR as an object lesson. Both his analysis of Russia (capitalist encirclement, etc., but no reference to traditions of Czarist bureaucratism or revolutionary organizationalism) and his proposed political institutions (workers' councils, etc.) are familiar. His conclusion is that the conditions which produced the Stalinist "malformation" in Russia are absent in America, hence that the third American revolution poses none of the threats to freedom which liberals envisage.

One problem with all this is the introduction of politics as a dominant factor. A deeper one is the absence of any detailed examination of historical situation and process. Marx's adjuration to proceed from the earth to the heavens seems forgotten. And Novak's political desires appear problematical indeed when one reads his depiction of the coming American revolution—"the direct struggle for power between the armed camps will be exceedingly ferocious, hard-fought, and protracted" (p. 271). Perhaps the kindest epitaph for Novak's political design is Marx's old one—"idealist." In activist as well as scholarly writing, hope and conviction are poor substitutes for analysis. (A final comment on the text: my copy included a double printing of ten pages, out of order—an easily corrected error.)

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Studia Semitica. Volume II: Islamic Themes. By Erwin I. J. Rosenthal. (Cambridge: Cambridge University Press, 1971. Pp. 222. $9.50.)

The second volume of E. I. J. Rosenthal's collected papers is devoted to Islamic themes, specifically political philosophy in medieval Islam and religion and politics in modern Islam. Before commenting on Professor Rosenthal's works, I should like to offer few remarks on the development of Islamic political thinking to help set his contribution in context.

Traditional Islamic political theory is expressed in several different literatures. Islamic lawyers and religious thinkers have devised a theory of the Caliphate. From the Persian tradition of statecraft comes the genre of "mirrors for princes." From the Greek heritage stem the Islamic commentators on Plato and Aristotle and their notions of the ideal city. Each of these literatures embodies its own tradition of discourse, characteristic concerns, and literary metaphors, but they have in common certain essential problems. Each is concerned with the relationship between ultimate values and the organization of society, and in each tradition the general problem is explored in terms of the ideal ruler and his relation to society and to religion. The importance of the philosopher-king in the Platonic tradition needs no comment here. In the Persian inspired genre, paradigmatic anecdotes show how the ruler maintains justice, order, and virtue by grace of his perfect actions. In Islamic legal and religious terms, the selection, qualifications, and responsibilities of the Caliph are the central concern. Political thinking, in each of these genres, really means the speculation about the roles of the ruler in the organization of a society which make possible the fulfillment of the basic religious or other values of that society. Politics and religion, as is commonly said of Islam, are inextricably interrelated.

Islamic political thinking is essentially value-oriented. Empirical observation, pragmatic advice, or operationally valid hypotheses about political realities play but a small part. As such this literature will appeal more to historians of culture and society than to analytically minded political scientists, but insofar as some aspects of political and social science research take into account the place of social values in the formation of social institutions and social actions, value-oriented literature may be a key to the meaning of social and political action as experienced in a particular culture. This literature says little about the dynamics of political institutions in Islamic countries, but it says a great deal about the meanings and purposes for which Muslims engage in political actions and what they expect of political institutions. From a functionalist point of view politics may have analogous functions in the operations of any society. From an existential or value bearing point of view, politics is a quite different experience in the life scheme of different cultures.

Professor Rosenthal's contribution to the subject is a selection of articles written between 1940 and 1967, a selection somewhat superseded by the author's own *Political Thought in Medieval Islam* and *Islam in the Modern National State*. Of the articles in this volume, the second, "Some Aspects of Islamic Political Thought," introduces the three main Islamic genres of political thinking and sketches the views of the several philosophers treated at length in other articles. The other essays in the first section are devoted to al-Farabi, Ibn Bajja (Avempace), Averroës, and Ibn Khaldun, and to two brief studies on the concepts of *eudaimonia* and of prophecy in Islamic philosophic theory. Professor Rosenthal analyzes the political theories of the various thinkers, their interrelationships, similarities, and differences of perspective. Muslims as well as philosophers, these thinkers maintained a constant dialogue between Islamic and Greek thought. In each tradition they found similar goals and concepts. Their central concern was to understand the relationships between religion and philosophy, revealed and rational truth, prophet and philosopher.

As Professor Rosenthal shows, these thinkers were devoted to an ideal of human perfection in which intellectual vision was carried out in moral action. Greece and Islam were agreed that virtue and knowledge, though differently defined, were inseparable. To realize this perfection, the correctly organized state was essential (to all except Ibn Bajja). To be perfectly organized, the state required the leadership of a philosopher-*imam* who himself embodied the completeness envisaged in each tradition, and the state had to be regulated by wise laws—*Shari'a* in the Islamic religious tradition; *nomos*, in the Greek. Even Ibn Khaldun, whom Professor Rosenthal sees as the only realistic and empirically-minded Muslim thinker, recognized the crucial importance of religious values and laws for the formation of the state. Thus, to the philosophers who sought to harmonize the two traditions, Greek and Islamic thinking were essentially congruent, and Greek thought served as a model for the interpretation and evaluation of Islamic situations. Professor Rosenthal's contribution is to explore the Islamic philosophers' own explorations of the interrelationships between Greek and Muslim imagery for the perfect state and the perfect human being.

In the second part of the book, Professor Rosenthal takes up the contemporary situation. The confrontation is no longer between Greece and Islam, but between the modern West and Islam, and the question is, how are traditional Islamic conceptions of the Muslim state and of the role of religion in society to be reconciled with the development of secular national states? In this section of the book, Professor Rosenthal reviews conditions in Pakistan and Malaya and the theories of recent Muslim writers on politics. Neither in thought nor action, is it clear, has any viable reconciliation of traditional values and modern conditions been devised. Muslims are still generally unable to resolve the conflict between the ideal of an all-embracing Islamic state based on traditional religious law and the reality of secular states in which Islam is no more than the religion of private persons. This is not unusual, after all. As in ancient times, the tension between ideal values and political actualities seems intrinsic to Islamic societies.

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The Logic of the Law. By Gordon Tullock. (New York: Basic Books, Inc., 1971. Pp. x, 278. $7.95.)

Though it is perhaps the most natural thing in the world to view "law" as a set of rules, this perspective has made few inroads in the behavioral sciences. Law-as-rules has been a staple of recent legal theory, prominent in the work of Lon Fuller, H. L. A. Hart, and Gidon Gottlieb, among others. Yet aside from occasional incursions, social scientists in general—and political scientists in particular—have remained largely oblivious. We have preferred to speak in terms of "judicial process" or "legal policy," not in terms of anything so apparently nebulous as rules. This bias may be chalked up to the pervasive and often unacknowledged influence of the extreme legal realists, whose rule-skepticism continues to shape our thinking about legal decision making.

Much the most interesting aspect of Gordon Tullock's recent work is the seriousness with which he takes rules, a seriousness manifested in the very title of the book. Indeed, if I read him correctly, Tullock argues that the failures of legal institutions can often be ascribed to a general failure to invest legal rules with the proper degree of logical rigor. The imperfect fit of law and behavior thus is ascribed to be a lack of fit of legal rules and individual decision making. If rules were formulated out of an awareness of the well-springs of behavior, the consequences of those rules would be more in line with legislators' expectations.

Tullock approaches his analysis with, as might be expected, the analytic tools of welfare economics. Thus, his argument begins with three assumptions familiar in economic analyses of politics: that individuals are not in positions to assure themselves special legal treatment; that individuals choose what they prefer; and that individuals normally prefer choice to no choice. By way of building upon these assumptions, Tullock proceeds to detailed discussions of topics in both civil law (such as contracts and accidents) and criminal law (such as motor vehicle offenses, tax evasion, theft, and fraud).

The strong emphasis upon civil law—more than a third of the book—constitutes a striking and promising departure from the run of social science legal analysis. Whether he is drawn by his own legal training or by the susceptibility of civil law subjects to economic analysis is difficult to say. Whatever the motivation, the result is one of the comparatively rare instances in which a social scientist enters in a sustained manner into the interstices of private legal relationships. The conventions of political science declare private law to be out of bounds, yet in terms of the impact of law upon individual behavior, it is at least arguable that this impact occurs most frequently in the sphere of civil law. Thus we are in Tullock's debt for his sustained attention to such relatively unglamorous subjects as the law of contracts. The term "public law," already vaguely anachronistic to recent students of judicial behavior, serves now principally as a set of blinders, deflecting scholarly perceptions away from those areas of law presumed to be devoid of political content or interest. That "contract" should function as a legitimate concept in political theory while being effectively exiled from public law is an irony, which Tullock goes some distance toward resolving.

All of this having been said, the book remains flawed in a number of significant respects. In the first place, it fails to give any feeling of a continuing, sustained argument. The three fundamental assumptions are doubtless meant to provide a unifying thread, as is the application of techniques of economic analysis. Nonetheless, the book seems fragmented. It is less an integrated theory of law than a set of relatively discrete studies. Although the studies together touch upon a broad range of both substantive and procedural issues, the overall impression remains one of a single technique rather than of a single web of argument. The chapters on, for example, motor vehicle violations and on the criminal process are interesting for themselves, not for the manner in which they might interlock with other parts of the book.

The fragmentation may have been in part an artifact of method. Tullock has chosen to examine those segments of the American legal corpus most likely to be illuminated by economic concepts. Inevitably, he has been drawn to those in which possible outcomes can, without too much difficulty, be conceived in monetary terms. Commercial areas naturally come to mind most readily, as well as crimes against property. Crimes against persons receive brief attention, and Tullock admits that the area proves refractory to welfare economics.

Intellectual insularity has always characterized economic approaches to political science. The premium has been placed on internal completeness and consistency, and only secondarily upon the reception of supportive evidence. In this respect, Professor Tullock's book is both representative and equivocal. The arguments have obviously been constructed with an eye toward their internal cohesiveness, and most of the time we are asked to accept the conclusions principally on the ground of their compelling logical force. From time to time, however, Tullock moves from argumentation to the assembly of evidence, most notably in his analysis of criminal justice. My quarrel with the book lies less in the quality of evidence than in the inconsistency with which it is utilized. Even policy preference does not appear to govern the appearance of data; it is simply inserted as if by chance. Consequently, much research that might be of value—for example, empirical studies of the efficacy of sanctions— is never drawn in. The author seems unable to decide whether the work's persuasive character should derive solely from the arguments or whether it might best result from a fusion of logic and data.

The tenuous and ambivalent connection between the substance of *The Logic of the Law* and the literature of the field creates other problems as well. Thus Professor Tullock offers a number of remedies to deal with alleged inequities and inefficiencies in the criminal justice system: the abolition of the jury, relaxation of exclusionary evidence rules, and so on. I take issue not with the merits of his proposals, but rather with the lack of novelty of many of them. Indeed, a comparable set occupied the attentions of Jerome Frank in *Courts on Trial* (1949).

Finally, I find it difficult to share Professor Tullock's frequently expressed view that his arguments and conclusions will scandalize, affront, or outrage his audience. Even given the fact that the book appears directed at both professional and lay readers, Professor Tullock's delight in scandal seems misplaced. Indeed, nothing in the book appears likely to do more than raise an occasional eyebrow. Professor Tullock's insistence on the scandalous character of his observations ultimately stems from his desire to remain rigorous, hardheaded, and free from intellectually debilitating sentimentalism. Laudable though these aims may be, the result in this case is neither shocking, nor revolutionary, but simply a stimulating, lucid, if somewhat disjointed exploration of the American legal system.

MICHAEL BARKUN

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Children and Civic Awareness. By Charles F. Andrain. (Columbus, Ohio: Charles E. Merrill Publishing Company, 1971. Pp. 214. $3.95, paper.)

This is a report of a conventional questionnaire survey of elementary school children in an affluent California suburb in 1966, padded out into a book by wads of rather vague sociology and quick summaries of the political socialization literature. Like much of that literature, it displays at times a startling political naiveté, of which a sufficient sample is the author's belief that Britain is a "conservative nation" and Russia a "revolutionary nation." It shares also a characteristic disregard for the problems of validity—acute in such research, and exaggerated in this case by the author's having mixed factual and attitudinal items systematically through his questionnaire. There is, however, a brisk attack on the contents of local school texts; if the question of how such texts got to be there had been pursued, the study might have been much sharper.

The book stands out from the ruck of political socialization work in one way. It is the first American book on the topic to take extended notice of Piaget. Andrain has studied Piaget carefully and develops a number of interesting ideas about the application of his theoretical categories to the development of political consciousness. Unfortunately he does not appreciate fully that Piaget's thought, like Freud's, is closely linked to a particular method. A Piaget-type interview is an interactive test which produces evidence of a kind very different from that of forced-choice questionnaires. It is extremely difficult to make a convincing link between orthodox questionnaire material and Piagetian theory. Andrain's work does not succeed in making the empirical connections because, having chosen questionnaire format, he does not include any measure of Piagetian formal thinking. The studies by Adelson, O'Neil, and Gallatin remain the only American research on politics which effectively uses Piaget. (See Joseph Adelson and Robert P. O'Neil, "Growth of Political Ideas in Adolescence: The Sense of Community," *Journal of Personality and Social Psychology*, 4 [1966] 295–306; Judith Gallatin and Joseph Adelson, "Individual Rights and the Public Good: A Cross-National Study of Adolescents," *Comparative Political Studies*, 3 [1970] 226–242.)

Of the orthodox questionnaire findings, the most interesting is a failure to find much in the way of sex differences.

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Obiter Dicta: Opinions, Judicious and Otherwise, On Lawyers and the Law. By Joseph W. Bishop, Jr. (New York: Atheneum, 1971. Pp. xii, 298. $8.95.)

Obiter Dicta is a collection of essays dating mainly from the late '50s and '60s on lawyers and the law. The title is legalese for extraneous statements, and announces that the essays are not central to the serious scholarship of their author, Joseph W. Bishop, Jr., who teaches law at Yale. Most of the essays were originally published as book reviews in professional journals or articles in popular magazines.

The general subjects of the essays are litigation, litigators and litigants, and some issues under the Constitution, in torts, crimes, evidence, military and international law, which have received popular attention in recent years. The author states (p. vii) that there is no unifying theme to the collection, except their relationship with law. This is essentially correct so far as the subject matter is concerned. There is, however, an underlying premise to each of the essays which unifies the whole sufficiently. This premise is that there is a lot of bunk flying around; Mr. Bishop takes it as his task to debunk, through analytical exposition.

A common difficulty in executing the art of debunking is that passion occasionally overtakes rigor. Some of the essays have this problem, most notably "Electronic Eavesdropping and the Public Interest" (pp. 180–196). This article, which appeared in June, 1969 in *The New York Times Magazine*, lays out the constitutional history, principal acts of Congress, and Court decisions bearing on bugging and wiretapping. While this is admirably done, the article fails to include any discussion of whether the disadvantages of wiretapping are mitigated by much in the way of demonstrable benefits from the practice. This omission would not be troublesome except that the article concludes with the author's opinion that legislation then current struck a fair balance between the needs of police and the rights of citizens to privacy.

There is almost nothing in the body of the essay to support this conclusion. The flavor of this conclusion and consequently Bishop's exposition do nothing to ameliorate my own distaste for the practice.

Often the author outlines a position between polar propositions which were at the time of writing subject to active debate. In so doing, he tends to find more on his left to deflate than on his right. He is especially instructive on subjects which cause liberal knees to jerk in unison—subjects such as military justice and wiretapping, on the one leg, and the affairs of Counselor Kunstler, Dr. Spock, Reverend Coffin and the "Warren Court" on the other. He finds these to be appropriate for some thoughtful analysis even if it means depositing a little calcium here and there, or twanging a few tendons. He advocates his position with no less passion than those who are participants in the litigation or advancement of the issues involved. Since moderate positions rarely get public attention, the author performs a highly useful function in presenting them, the more so since Bishop is very skillful in getting both facts and law straight, and in writing them up with balance and clarity. To the extent he focuses on actors instead of issues, however, he tends to ignore the fact that they are often bent on achieving meritorious social goals, despite the somewhat distasteful excesses of their behavior.

No book review, including this one, is complete unless the reviewer quibbles with the author over some point of more or less gravity. This is especially appropriate in view of the collection of outrageous (and very funny) quibbles in Bishop's own reviews. For instance, in his review of *The Trial of Dr. Spock* he twits writer Jessica Mitford for incorrectly installing a maid in the home of his colleague, Abraham Goldstein (p. 162, footnote 3); in other essays he allows that he can think of no one other than Ezra Pound who may fairly be classed as an American egghead Fascist or sympathizer (neither can I) (p. 153); propounds his considered view that the derivation of the cant term "nark" is "from the Romany word 'nak,' meaning nose" (p. 195) and distinguishes Thurman Arnold from other revolutionaries, such as Lenin and Mary Baker Eddy, in that the latter "are united by their total lack of humor" (p. 71). While I have no doubt concerning his conclusion on Thurman Arnold, we have the testimony of Maxim Gorki that Lenin had a keen sense of humor, enjoyed fun and could laugh uproariously. See David Shub, *Lenin,* Doubleday & Co., Inc., Garden City (1951), page 367. This testimony is corroborated as to the young Lenin in Bertram D. Wolfe, *Three Who Made a Revolution,* Beacon Press (paperback), Boston (1955) at page 55. In the recent profile of Julius Nyerere by William Edgett Smith in *The New Yorker,* October 16, 23 & 30, 1971, there were many references to the sly humor of a contemporary revolutionary.

Indulging in some *obiter dicta* himself, Cassoday, J., writing his opinion in Buchner v. Chicago, M. & N.W. Ry. Co. (19N.W. 56, 58; 60 Wis. 264 [1884], noted that ". . . mere *obiter* [*dictum*] is not always reprehensible. . . To be more valuable, however, they must, of course, be right." The essays fit well within Judge Cassoday's first statement—they are not always reprehensible—in fact they are not even reprehensible most of the time. And the points Mr. Bishop makes are more valuable, by application of the second half of Judge Cassoday's statement, since mostly, he is right.

While each reasonable man will find much with which to agree and disagree in *Obiter Dicta,* it makes satisfying reading and is on the whole a respectable body of work. Particularly engaging in it are personal opinions and asides by the author, in which he makes disarmingly honest statements about his own views, motives and doubts. It adds up to a good book, purposefully hardheaded but for a Yankee author, adequately softhearted.

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Dissent and Disruption: Proposals for Consideration by the Campus. By The Carnegie Commission on the Future of Higher Education. (New York: McGraw-Hill, 1971. Pp. 309, including 19 appendices. $4.95.)

The university revolt, which by common consent dates back in this country to the Free Speech Movement at Berkeley of 1964–65, is seven years old, and the American academic community is now bestirring itself to restore campus calm and to systematize institutional change before the first full decade has elapsed. This, the ninth in a series of relevant reports by the Carnegie Commission on the Future of Higher Education, is therefore a timely publication.

Dissent and Disruption consists of a "model bill of rights and responsibilities for faculty, students, administrators, staff, and trustees;" an extensive explanatory report with proposed recommendations; and some sixty factual exhibits and informative appendices, including other codes of college conduct and university regulations. Though it reiterates certain findings and conclusions of the President's Commission on Campus Unrest of 1970 (the Scranton Commission), the Carnegie volume constitutes a refreshing body of speculation and a revealing set of documentation about the governance of institutions of higher learning during the current crisis and for the crucial years ahead.

The book devotes itself faithfully to both aspects of the dual problem of protecting dissent and preventing disruption. Nevertheless the reader can readily discern the Commission's emphasis on the latter. "Academic freedom," the Commission affirms, "is now under attack from within the campuses as well as from outside . . . (While) dissent is properly protected in a democracy, disruption must be sternly condemned, suppressed, and punished" (p. 6).

The Carnegie Commission singles out for special protection "the fundamental rights of others" (presumably those of the nonactivist or silent majority), listing these rights first in the general preamble of its recommended code of conduct. Subsequently the Commission's report deals more concretely with these "cases of interference with the constitutional rights of others." In attempting to restore respect for these basic rights, the Commission spells them out as follows (pp. 38–40): "freedom of members of the campus to pursue normal academic and administrative activities, including freedom of movement"; protection from "physical harm, threats of harm, or abuse"; and the right of "privacy in offices, laboratories, and dormitory rooms and in the keeping of personal papers, confidential records, or effects." When such normally established personal and professional rights have to be codified for members of university communities, campus life in America is portrayed—and no doubt accurately—as precarious, indeed. Sedate political scientists today may well wonder how a possibly more restrained posterity will react to the report's almost prosaic enumeration of "beatings, rock-throwings, and destruction of buildings," not to mention "terrorism," which the Commission laconically defines as "planned, deliberate, acts of violence by clandestine groups," and which it unabashedly exemplifies as "bombings and arson," (p. 6).

Since the Commission anticipates continued disruption, it stresses for university administrators the need to provide "effective measures for consultation and contingency planning in the event of disruptive emergencies" (p. 65). Although it finds the prospect "repugnant," the Commission advocates "closing the campus in the face of continued violence or unrelenting and potentially dangerous nonviolent disruption" (p. 68). It recommends some extraordinary administrative measures which may prove necessary in such contingency, ranging from the adjustment of academic credit for students or curtailment of salary payments to faculty to the continuation, during an emergency shutdown, of "care of hospital patients and of laboratory animals." While providing such emergency planning for recurrent crises, universities are also urged by the Carnegie Commission to put their trust in "more formal codes, more clearcut decision-making processes, and more independent judicial procedures" (p. 95).

The Commission assumes, however, that despite such anticipatory arrangements, order on campus will not be successfully maintained without occasional appeal to the civil authorities or residual recourse to "the general law." In dealing further with this controversial question of campus policing by the off-campus authorities, the code boldly states, "the campus is not a sanctuary from the general law." The report reiterates this assertion in four separate passages, but in the light of unpunished infractions still occurring on or near American campuses, this assertion is clearly an admonition rather than a canon of academic conduct.

The Commission's search for a more orderly day-to-day existence on the American campus is reflected not only by its attempts at such built-in safeguards to protect the so-called silent majority, but also by its readiness to confirm rights already gained by campus dissenters and demonstrators in the course of the ongoing revolt. The Commission's report expressly recognizes the right to engage on campus in "peaceful non-obstructive sit-in" and it equates the sit-in with a "sedentary picket line" (p. 6–7). Furthermore, the Carnegie code recognizes that the university now has an obligation "to accept legal strikes legally conducted without recourse to dismissal of participants." Still, it retains for the university the right "to deny pay and academic credit to members of the campus who are on strike," arguing convincingly that "the campus should not make claim to be the only area of society where strikes are cost-free to participants" (p. 41).

The Commission concedes that the now widely accepted "time, place, and manner" rules constitute the maximum permissible regulation of organized expression on campus, thus absolving from university control the substance of on-campus activity generally as well as the content of advocacy specifically. It does this even though the time-place-manner formula, borrowed during the Berkeley Free Speech movement from the remote realm of federal-state electoral law, has failed to adequately regulate the increasingly sophisticated forms of confrontation and coercion practiced on college campuses.

In its causal analysis, the Carnegie Commission assigns prime significance to the political milieu, both the burgeoning political community inside the university and the broader body politic outside. But it goes deeper and identifies the underlying socioeconomic as well as political causes for campus disorder. In thus explaining the "dissatisfaction and disaffection" which prevail on campus, the Commission quotes freely from the Scranton report. Accordingly, it lists today's unsolved "problems of American society" as follows: "achievement of equality of opportunity for all American citizens, ending the war in Vietnam, elimination of poverty, preservation and restoration of our physical environment, assessment and control of the impacts of technology, and responsiveness of our institutions to the needs of the people" (p. 16). If this imposing enumeration is to be accepted as the Megillah for all our academic troubles, it is hard to see how even the Carnegie Commission's resolute report and sage recommendations can effectually control or constrain campus disorder. The most that can be expected in such an imperfect world, in which the universities are in a state of permanent turbulence over pending societal reforms, is that the Carnegie type of proposals will at best serve to contain and codify campus dissent and disruption.

In the more delicate realm of political principle, the Carnegie Commission's deliberative search for behavioral and procedural proprieties on campus is more viable. The Commission recognizes both the "right and obligation" of the university "to provide for members of the campus the use of meeting rooms under the rules of the campus, including use for political purposes such as meetings of political clubs." But at the same time it grants to the university the right and obligation "to prohibit use of its name, its finances, and its office equipment and supplies for any political purpose at any time." And this strict prohibition, despite the granted right to the use of meeting rooms, is also applied against "use of its rooms by individual members or groups of members on a regular or prolonged basis as free headquarters for political campaigns."

These discriminating demarcations between proper and proscribed forms of political activity on campus are derived from experience, particularly from the Free Speech Movement at politically-conscious Berkeley when the instigating issue was the right to engage in legitimate political activity. It is not surprising, therefore, that these provisions of the Carnegie code reveal the fine hand of Commission Chairman Clark Kerr, formerly Chancellor at Berkeley and later President of the statewide University of California system. In fact, several other crucial passages of the Carnegie code and the Commission's recommendations are warmed-over and up-dated restatements of proposed guidelines for campus conduct which Kerr had then favored but had failed to sell to his capitulatory faculty. Ever since then at Berkeley, as elsewhere, faculties have been trying to build backfires against excessive campus activism.

With the present reduction in voting age, newly enfranchised student voters may learn how to circumvent even more subtly the codified regulations which may be imposed upon their overzealous political activities. On the other hand, their newly won political muscle may in the future come to be expressed in a reverse flow of political power from the campus to the community, possibly inducing students to renounce use of the university campus in favor of the party headquarters as a renascent staging-ground from which to mount their off-campus political campaigns.

Thus far in the revolution, it seems, the level of on-campus political debate has been less instructive than inspiring, and often it has not even been a cut above the lower forms of political disputation on the off-campus hustings. No code of academic procedures and no regulations of campus conduct, even ones as wise and workmanlike as those produced by the Carnegie Commission, can repeal such a Gresham's Law of politics, once the worse begins to displace the bad. All this, therefore, offers a new challenge for teachers of political science, and for university faculties generally, to resume the arduous task of transmitting acceptable standards of discourse and imparting appropriate styles of thought to those of their students who are once again willing to listen and to learn.

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Financing the Metropolis: Public Policy in Urban Economies. Edited by John P. Crecine. (Beverly Hills, California: Sage Publications, 1970. Pp. 632. $20.00.)

Only in recent years have economists begun to take time from their more traditional concerns to look seriously at the structure and problems of the urban community. The first text on urban economics did not appear until 1965, and it was not until 1968 that efforts to mark out the field conceptually materialized in a series of papers edited by Harvey S. Perloff and Lowdon Wingo (*Issues in Urban Economics,* Johns Hopkins Press). Even the urban public economy received little serious intellectual attention from the discipline prior to the last decade, despite the long standing fiscal difficulties of American cities. Happily, efforts such as those represented by the present work are now under way to remedy this neglect.

Financing the Metropolis, the fourth volume in the Urban Affairs Annual Review series, comprises twenty individual essays, fourteen of them written by economists. The collection offers little that is new to the student of contemporary communities. What it does provide, however, is a coherent set of readable papers that examine some of the basic urban issues now occupying the attention of economists and other social scientists. In the process, it also suggests a number of policy alternatives designed to improve the quality of urban life. The economics is presented in such fashion as to be readily understood by those outside the discipline.

Although they are of mixed nature, the essays hold together well. Their organization in the volume follows a logical sequence that starts with an overview of the urban economic system, runs through the issues of the local public economy, and concludes with the operation of the service delivery system. The first of four parts that make up the book discusses the nature and characteristics of the urban economy and the role government plays in it through interventionist policies and the direct provision of public goods. The second deals with the allocational process of the urban polity and the factors that influence revenue and expenditure patterns. The third examines various facets of the ghetto economy and the manner in which the poor are affected by current public policies in such areas as housing, employment, and taxation. The final section looks at the provision of public goods and services and the way they are delivered to the citizen consumers, again with particular reference to the deprived. Although the selective focus in these latter two parts is dictated primarily by space limitations, it can be justified intellectually because of the overriding importance of poverty in the economic pattern and problem inventory of the cities.

The section dealing with the urban public economy merits particular attention since, taken in its entirety, it constitutes the strongest and most comprehensive feature of the volume. It is also the one of greatest interest to the political scientist because of its timeliness and policy orientation. The papers which comprise it (by Wilensky, by Mushkin, and by Meltsner and Wildavsky) center around three closely related categories of concerns: the correlates of local government expenditure patterns, the use of more systematic budgeting practices, and the issue of federal revenue sharing.

Wilensky's review of the numerous empirical studies relating to the determinants of resource allocation decisions by urban governments points up two facts: (1) the work has been of little practical assistance to policy makers since it has been limited largely to showing the general effects of various social and economic characteristics on expenditures (it is of little help for local officials to learn, for example, that the principal determinant of output levels is the wealth of a community); and (2) more consideration must be given to the political and bureaucratic mechanisms that generate resource allocation decisions if the policy relevance of research efforts in this field is to be increased.

Mushkin's paper examines in detail the basic elements of PPB (planning, programming, and budgeting) systems and calls for the expanded adoption of this device at the local level to permit a more effective and rational allocation of available resources among competing demands. Meltsner and Wildavsky, on the other hand, argue that the costs of PPB would far exceed the relatively minor benefits to be derived from its use. Municipal budgeting, as they claim, is less an allocational tool than an internal control device to assure that expenditures will not exceed income and necessitate new taxes.

Most students of public finance recognize the severe resource limitations under which local functionaries operate. In the face of these constraints, urban budgeting, no matter what its form, is essentially a process in which choice is restricted to the continuation of past programs and practices, with only small incremental changes possible from year to year. This situation conceivably would change if substantial new funds became available to local policy makers. Were this to occur, interest in more effective means of resource allocation at the urban level would rise appreciably. Here the papers by Riew and Fitch on federal revenue sharing, the largest potential source of new funds for localities, are pertinent. As both authors caution, such sharing without attached conditions might serve simply to perpetuate or magnify present inequities and inefficiencies in urban fiscal systems. Past experience certainly provides little reason to believe that municipalities and counties would disburse unrestricted funds under a systematic process of considered priorities. Fitch would therefore have any general-purpose grant used not only as a revenue-sharing device but also as a means of stimulating improvements in state and local government organization and planning. If such restrictions were incorporated into a revenue-sharing program and rigorously enforced by federal authorities—a not very strong likelihood at this time—PPB would assume new importance for local units as a management and allocation tool.

Discussions about fiscal solutions and reorganization of urban governmental structures often fail to distinguish between income redistribution and service-providing objectives. This failure almost inevitably leads to conceptual ambiguities in formulating proposals for change. As Sandalow's paper on federal grants shows, the issues concerning taxation and areawide governmental reform take on a different character if income redistribution is to be accomplished through national and state budgets rather than through heavy reliance on local service delivery systems. If the federal government, for example, were to assume the full costs of public education and welfare, the redistributive importance of local revenue and expenditure patterns would diminish significantly. This result would, in turn, materially affect the kind of taxation policies and metropolitan governmental arrangements that reform advocates might logically opt for in the nation's urban settlements.

The contributions to the volume, such as those cited, illustrate how the tax issues that occupied earlier students of urban public economy have given way to the more intricate problems of distributive justice and relative service burdens and benefits. They also indicate the large role that policy concerns are playing in the development of the field of urban economics. This is an encouraging and welcome trend.

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The Rights of Americans. Edited by Norman Dorsen. (New York: Pantheon, 1971. Pp. 679. $12.95.)

To commemorate the fiftieth anniversary of the American Civil Liberties Union, Norman Dorsen has edited a volume whose thirty-one essays combine breadth of coverage with depth of legal research, penetrating analysis of the law, provocative justifications for legal change, and clear writing.

Dorsen has ordered them under five major topics of contemporary libertarian concern: "the right to the essentials of life" (e.g., welfare, housing, employment, education); "the right to influence government and public opinion" (voting, publishing, participation); "the right to personal autonomy" (religious liberty, drugs and alcohol, travel); "rights against governmental process" (rights of suspects, juveniles, prisoners); and "rights of particular groups" (students, teachers, women). And he has recruited distinguished civil libertarians as authors—Thomas I. Emerson, Harry Kalven, Jr., John de J. Pemberton, Jr., Leo Pfeffer, Leonard Boudin, Roy Lucas, Michael Tigar, Anthony Amsterdam, and William Van Alstyne, to mention a few.

Intriguing variations mark these essays. Emerson, Kalven and Van Alstyne, for instance, confine themselves almost entirely in tight legal analysis. Charles Lister includes a glimpse of the history of abortion regulations to show their obsolescence in changing times. Edward Sparer appeals for a broadly based political campaign to vindicate welfare rights; and he admonishes welfare lawyers to limit themselves to technical assistance, leaving strategy to recipients. Graham Hughes ponders the ethical and utilitarian justifications for preferential treatment, while Bruce Ennis painstakingly bares the inconsistencies in the "danger" and "welfare" theories justifying the commitment of mental patients. Pauli Murray's chapter on women's rights has symbolic significance as the only one written solely by a woman!

Some of the views advanced will unsettle even such ACLU members as this reviewer. James C. Kirby, Jr., seems to concede the vested privilege theory of voting: "In our complex system of governmental decision-making, it cannot be said that all property qualifications for voting are dead" (p. 179). And Howard I. Kalodner believes the failure of community control of schools was due to an overbroad franchise, for "the only electorate which could hold a school board accountable on a continuing basis was the electorate of parents" (p. 205). These arguments deny the tendency of a decade to assure the equal status of every citizen in public affairs.

Also disappointing is the inattention to private threats against liberty. Individuals and movements can also censor speech and coerce association. Civil libertarians will not proudly remember the studied unconcern or quiet sanction with which they greeted the silencing of prowar speakers, advocates of repugnant racial theories, and others with views held "immoral" during the late campus revolution. Yet there is scarcely a word here about such invasions of freedom.

This foreshadows a more general criticism, best expressed by Clyde Summers: "What is remarkable, if not disturbing, is that . . . extensive protection of the democratic process in industrial institutions has generated so little serious consideration as to how and in what degree individual rights should be protected within other institutions of private power" (p. 616). The rights of individuals in religious associations, business corporations, and occupational associations are virtually ignored. The due-process and free-speech rights of teachers and students are considered, but not their role in institutional decision making. The chapter on the right to participate focuses solely on governmental arenas, ignoring even such quasi-public institutions as political parties.

Some rights stand effectively beyond the reach of judicial protection, a theme elegantly expounded in Anthony Amsterdam's essay on the rights of suspects. He cites the shortcomings of the exclusionary rule in reaching police conduct not directed toward producing evidence. And he warns of the slight impact that Supreme Court decisions have on the tight network of judges, prosecutors and defense counsel at the trial level. Most important, when the Court attempts to act in areas not already laced with administrative regulations, legislation, and lower court rules, "it is seriously impeded in understanding the nature, purposes, and effects of what it is reviewing" (p. 407), and it cannot fashion effective remedies.

Indeed, this book's clearest message is the dominance of statutes and administrative rules in protecting liberty. A random flipping of pages reveals major references to the Criminal Justice Act, the National Environmental Policy Act, the Economic Opportunity Act, the Demonstration Cities and Metropolitan Development Act of 1965, the Voting Rights Act of 1965, the Federal Communication Act, the Omnibus Crime Control and Safe Streets Act of 1968, the Equal Pay Act of 1963, and a host of others. The text and notes are studded also with federal and state regulations, commission reports, and model acts. Will any political scientist ever again feel justified in teaching constitutional law from a casebook?

This volume has separate meaning as a manifesto. Despite Dorsen's disclaimer, it undoubtedly reflects much current sentiment in the ACLU, one of the most active and successful interest groups in the judicial arena. A changing Court may foreshadow harder times, as several essays warn, but students of the judicial process will nonetheless glimpse here the future course of ACLU advocacy.

A last, if unconventional, laurel is due—to the publisher. It seems unlikely that a book of this complexity, size, and price will return a profit. Yet Random House judged both the occasion and the book worthy. Virtue here deserves to be more than its own reward; and it should be if all read this book who ought to.

David Adamany

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Urban Problems and Prospects. By Anthony Downs. (Chicago: Markham Publishing Co., 1970. Pp. 293. $9.95, cloth; $3.95, paper.)

Downs's book is a collection of eleven papers on urban problems ranging from racial segregation to peak-hour expressway congestion. Written over an eight-year period, most of them have been published previously, but in such a variety of hard-to-peruse sources that their collection in a single volume will certainly be appreciated. Downs has selected the eleven papers from among a great many more of his published and unpublished writings. The mix of topics appears quite deliberate, therefore, as does the generally high quality of analysis aimed at by the collection. Whether intended or not, the collection also has the great virtue of minimizing internal redundancy: sentences and paragraphs do not get served up two or three times, separated only by the white spaces between one chapter and another.

Although Downs's real genius lies in his virtuoso style of attacking a problem, his substantive sophistication is hardly less impressive. How can lawmakers and other concerned citizens be persuaded to fight for the adoption of school evaluation systems? Recommend the analogy of public auditing by outside accountants in seeing that funds are being used wisely (p. 284). Although "new cities" are not to be counted on to house a large fraction of our growing population, a crucial tactic in building them is to plan land uses "so their spillover effects reverberate back and forth within the boundaries of the new city, thereby accelerating the process of escalating land values there" (p. 239). As more and more landlords are forced to abandon their buildings to their local governments because of the cost of complying with building code standards, "we could conceivably wind up with thousands of poor households living rent-free in government-owned urban slums" (p. 163). Finally, we may note one of Downs's overarching premises, "the need to use the dynamic economic expansion of suburban growth as a major input to solving inner-city problems," or linking a city's two principle frontiers, the "Frontier of Deterioration" and the "Frontier of Growth" (pp. 10–11). The elaboration of this premise leads, throughout the whole book, to a rich array of policy suggestions.

In his introduction, Downs declines credit for having invented the various analytical tricks he employs. Still, in his hands they are remarkably effective. One is the concept of "alternative futures," along with its auxiliary concept, "social strategies." An example will clarify their meaning and illustrate their utility. Downs delineates "five basic alternative strategies relevant to future development of ghettos," in his words: present policies, enrichment only, integrated core, segregated dispersal, and integrated dispersal. These five are defined as various combinations of outcomes on three dimensions—the degree of nonwhite concentration (or dispersal), the degree of nonwhite segregation (or integration), and the degree of enrichment provided "the most deprived groups" (pp. 40–41). This approach is characteristically Downsian, of course, as is evident to anyone familiar with his *Inside Bureaucracy*—grasping for a high level of abstraction while staying rooted in the concrete realities; simplifying in bold strokes while deferring to limits set by the natural variety of things; and generating an array of plausible constructs by the systematic cross-classification of dimensions, but discarding some (occasionally, most) as irrelevant or unreal.

With Banfield (as in *The Unheavenly City*) Downs shares an unusually intense concern with estimating the magnitude, or scale, on which "problems" exist. He goes beyond Banfield, however, in his readiness to describe what government and private enterprise would have to do in order to deal with these problems on their truly enormous scale. If 27.7 million housing units must be built between 1970 and 1980 to fulfill the officially proclaimed national housing goal, total investment required will be $654 billion. Downs argues convincingly that the financial institutions which have traditionally provided mortgage capital cannot conceivably provide this amount of capital unless annual consumer savings reach and remain at the very unusual rate of seven per cent. Government could help a little, though, by removing low ceilings on rates on mortgage loans to make them competitive with other investments.

The most striking *tour de force* in the volume is a paper called "Losses Imposed on Urban Households by Uncompensated Highway and Renewal Costs." Downs lists 22 such losses, assesses their impact on different population groupings ("low-income minority-group households" in particular), identifies those losses which it is both feasible and equitable to compensate, and estimates the cost of doing so. Conservatively the cost is—or was, until recent changes in federal law—between $156 and 230 million per year, and the deprivation falls on almost half a million persons annually. This, in Downs's phrase, is "injustice on a massive scale." It is a pity that other social philosophers and critics cannot—or will not—do sums as well as Downs.

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Federal Organization and Administrative Management. By Herbert Emmerich. (University, Alabama: The University of Alabama Press, 1971. Pp. ix, 304. $10.00.)

Classical public administration—including the school of scientific management—focused its concern upon organizational structure, legality, hierarchical control patterns, formal role definitions, and superior-subordinate relationships. Recent literature on administration and management has relied more on concepts from individual and social psychology. Interpersonal relations, motivation, conflict resolution, organizational socialization, styles of leadership, and informal channels of communication, for example, are principal concerns in the research and writing of Herbert Simon, Chris Argyris, Robert Golembiewski, Harold Guetzkow, and Anthony Downs. As Louis C. Gawthrop says, "Contemporary or 'modern' organization theory . . . , which has been variously referred to as the sociopsychological approach, the administrative *process* approach, . . . and so on, examines the organization in an altogether different context" (*Bureaucratic Behavior in the Executive Branch,* p. 31).

Professor Herbert Emmerich writes in the paradigm of classical public administration. Having made significant and lasting contributions to the President's Committee on Administrative Management in 1937 (the Brownlow Committee) and to the two Hoover Commissions, the author is in a good position to assess the pace and effectiveness of administrative reorganization from the 1930s through the Nixon administration. His style is largely descriptive. But the description supports Emmerich's basic thesis that the "Presidency is the focal point of any study of the interrelated aspects of executive organization and administrative management" (p. 10).

Too much of this book covers familiar

ground. A summary of U. S. administrative history before 1933 repeats information accessible in public administration texts. For richer detail, as Emmerich himself concedes, one consults Leonard D. White's multivolume study. Professor Emmerich's comparisons of Brownlow Committee and Hoover Commission goals and subsequent reports likewise have fuller treatment elsewhere; yet one's initial reaction against such reiteration is mitigated by the value of the author's personal accounts. His treatment of still other topics is often repetitious, failing to add much new insight to the work of Joseph P. Harris on legislative oversight, Emmette S. Redford on administrative responsiveness, Luther Gulick on principles of scientific management, Paul H. Appleby on the political bases of administration, Harvey C. Mansfield on presidential committees, and Morton Grodzins, for one, on cooperative federalism and administration.

As the title implies, the author's two principal concepts are organization and management. "[A]dministrative organization cannot be devised until clear decisions have been taken on what is to be done, at what levels and areas of government, and how it is to be financed" (p. 142). According to Emmerich, "[e]xecutive organization concerns those elements of structure, personnel, budgetary, and other systems of a government which produce an energetic and viable administrative management, capable of responding to the will and needs of the country and its citizens" (p. 102).

And what is the nature of management, the second concept? "The high tasks of administrative management are threefold: to recommend and espouse wise and just policies, to see to it that they are translated into effective programs, and to make sure that the implementation of adopted programs is vigorously carried out" (pp. 179–180).

At the intersection of organization and management lies the American presidency—an institution which Emmerich helped to shape. It is from the presidential perspective, not surprisingly, that the author casts his conclusions. General statements such as those in the preceding two paragraphs, designed to identify and interrelate the two most important concepts, raise difficult questions not only for presidents and administrators, but also for philosophers and political scientists. Emmerich does not create operational definitions or a set of explicit hypotheses. But he does offer some "tests of effective reorganization":

> (1) Is administration made constantly more responsive to the general interest of the country and better able to meet its ever-changing responsibilities and needs? (2) Is the ability of the President to make far-seeing recommendations for the general welfare of the country strengthened? (3) Is his ability enhanced to see that the 'laws are faithfully executed' and that programs sanctioned by Congress are vigorously and fairly pursued? (p. 11)

The reader is left to determine how effective administrative reorganization has been since 1933. The author's general satisfaction with administrative change—especially in the presidential office—is apparent. Yet, with regard to difficult criteria like effectiveness, responsiveness, and far-sightedness, the analysis is not helpful. For example, the author defines irresponsibility from a Congressional perspective: "The directives of the powerful members of Congress may be responsive to unidentified pressures from constituents, campaign contributors, or organized lobbies. They may also spring from the fixed notions of a congressman as to how things should be run" (p. 145). But throughout the book responsibility as a criterion remains nebulous—much like "the public interest."

Although there is no dearth of scholarship on the New Deal period, Professor Emmerich's insights into the Roosevelt administration are interesting and valuable. "My conclusions deviate from the inherited stereotype about his [President Roosevelt's] administrative weaknesses" (p. 63). He builds a convincing argument about the President's leadership ability—an argument bolstered from numerous perspectives: hiring and firing, interest in detail, staff work, and career officials. Emmerich's information lends further support to the conclusions of Richard Neustadt, Arthur M. Schlesinger, Jr., and Clinton Rossiter concerning Roosevelt's administrative leadership ability.

In conclusion, Emmerich looks to the "institutional presidency" for organization, reorganization, and management. In his last chapter, "Unfinished Business," Emmerich leaves little doubt that he is pleased with the growth of the presidential office and executive agencies. Moreover, he argues, one must look to the executive for greater unity of policy, both foreign and domestic. "It may be too much to expect the executive branch to become one completely united government establishment in view of its diverse centers of authority, power, and pressure; but, on its agenda of unfinished business, coordination must have a high priority." (p. 180) With such a statement for the future, Emmerich has told us much about his past.

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Labor in American Politics. By J. David Greenstone. (New York: Alfred A. Knopf, 1969. Pp. 408. $10.00.)

The purpose of this book is to argue that the close ties which have developed between organized labor and the Democratic party over the past twenty years represent the culmination of a historical trend, beginning in the 1930s. During this period, Greenstone argues, American labor has progressively given up the narrow, voluntarist, pressure-group tactics of the Gompers era in favor of a more collectivist, class-oriented type of political behavior which in some respects resembles that of the trade union component within the Social Democratic parties of western Europe. Or, to put it in Greenstone's own language, by the mid-1960s American labor had abandoned a traditional position of *articulating* its own limited, largely economic interest-group demands from outside the Democratic party, for one of helping to *aggregate* the general welfare ideology of the party from within. Later on in his book Greenstone uses this development to suggest the inadequacy of a pluralist interpretation of American politics as a whole.

Greenstone's empirical chapters describing the varying degree of integration of labor into the Democratic party structure in Detroit, Los Angeles, and Chicago, are excellently written and researched. It is when one examines the historical and theoretical implications of Greenstone's argument that questions begin to arise. Organized labor's developing partisan role during and after the 1930s, he argues, has been the result of "the super-imposition of an industrial economic order on a deeply entrenched democratic, procapitalist, and federal political regime." (p. 365) The industrial order of which Greenstone speaks, however, had been in existence in the United States long before the 1930s without producing the partisanship which he describes, as, indeed, had labor's attempts to influence the Democratic party toward the enactment of its legislative aims. This latter point is important, for in order to prove his case concerning the transformation of labor from pressure group to party constituent, Greenstone must show a qualitative change in labor's relationship with the party from the 1930s to the present day. Yet some of his evidence on this point is arguable, to say the least. In Chapter 9, for example, he describes the intervention of local Democratic party officials to prevent the nomination of C.O.P.E.'s choices for Congressional seats in his three sample cities in the 1960 and 1962 elections, arguing that labor's near success in nominating its candidates demonstrated its newly won power. Labor's inability to get its way, however, could equally well be read as a sign of a continuing subordinate, or special interest, role, rather than of a coequal or dominant one—the same role, moreover, that it had been playing in a number of industrial cities during the period of the New Deal. In addition, the discussion is couched almost solely in terms of labor's influence over the Democratic party's nominating or electoral procedures, with little in-depth analysis of its impact on matters of ideology or policy formulation, which would surely also be necessary to demonstrate an aggregating rather than an articulating role.

Greenstone's argument concerning American labor's partial assumption of a collectivist role within the Democratic party, similar to that played by labor in relation to the Social Democratic parties of Western Europe, is suggestive and convincing up to a point, but it, too, lacks the properly articulated comparative dimension necessary to make a convincing case. (This dimension, incidentally, could have been supplied at least in part by examining the work of Martin Harrison, Robert McKenzie, or Henry Pelling on the British Labor Party, or of Douglas Chalmers, David Childs, or Harold Schellenger on the German Social Democrats). It is true that in a number of states, American labor now fills a position in the Democratic party which is in certain respects analogous to that played by labor in the Social Democratic parties of Western Europe. But given the coequal (and in Britain, dominant) role which labor played in the founding of many European socialist parties, compared to the very different history of labor's role within the American two-party system, there is still little sign that American labor will ever play quite the indispensable institutional, financial, or policy-making role it plays in Europe. For all of its power in some of the industrial states, organized labor is still one among a number of constituents within the Democratic party, some of which (blacks, professionals, white collar workers) are now assuming a role of almost equal importance in the party; and it is at least arguable that labor's role, because of the apparent inability of the A.F.L.-C.I.O. to grow significantly beyond its 1940 levels, may become less rather than more important as time goes on. In Europe, on the other hand, without labor's financial and electoral support, most Social Democratic or Communist parties could not continue to exist.

Behind all of this lies Greenstone's interesting and subtly argued, but also in my view ultimately unconvincing, concluding attempt to refashion a theory of American labor politics in collectivist or neo-Marxist terms. He rejects on the one hand Marx's initial bourgeois-proletarian conflict model as too crude and simplistic for the United States; and he rejects on the other hand Dahrendorf's reformulation of Marx's model in terms of authority versus nonauthority role theory, as being too vague and fluid to say anything precise about power relationships in a highly industrialized society. Nevertheless, he then goes on to posit a model of class conflict, defined as that between economic authorities (producers) and economic nonauthorities (nonproducers or consumers, including labor unions), which is as open-ended and unsatisfying as Dahrendorf's own. "American unions in the mid-1960s," Greenstone argues, "were most likely to pursue their members' class interests on consumer issues, raised in national politics through the Democratic party's organized consumer constituency" (p. 375). The trouble with this concept is that it is never properly defined. If consumption means being a recipient of welfare services, or some such formulation, this would indeed provide some kind of basis for working-class unity, although hardly for far-reaching or dynamic social change. On the other hand, if by consumption is meant simply the consumption of goods and services, the concept is far too loose to have any meaning. American unionists, like American producers, play too many other roles in the society (as fathers, whites, blacks, taxpayers, churchgoers, etc.), some of which are in conflict with their role as consumers, for this concept to stand as a unifying force alone.

None of this should be taken to mean that Greenstone has not written an interesting and worthwhile book. As a serious and sophisticated attempt to re-examine labor's role both within the Democratic party and in American society generally, Greenstone's book can be highly recommended, even though it lacks a convincing historical and comparative dimension. As a vehicle for attacking pluralism, however, his case has yet to be proved.

JOHN H. M. LASLETT
University of California, Los Angeles

The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900. By Paul Kleppner. (New York: The Free Press, 1970. Pp. 402. $9.00.)

Younger historians, dissatisfied with the traditional narratives of presidential politics, have begun systematically to explore the social forces that animated American voting history. The Midwest has been the favorite locus of research, thanks to its abundance of small-unit electoral and demographic data and its highly developed party system. Kleppner's study, together with books by Frederick Luebke on Nebraska Germans (1969), Michael Holt and Bruce Stave on Pittsburgh (1969 and 1970), John Allswang and Joel Tarr on Chicago (both 1971), Ronald Formisano on Michigan (1971), this reviewer on the Midwest (1971), and an anthology of articles edited by Joel Silbey and Samuel McSeveney (1972), demonstrate that quantitative historical voting studies have come of age. Most important, these studies are in broad agreement about why people voted the way they did, and their conclusions must be weighed seriously by anyone studying American politics.

Religion and ethnicity emerge as the basic factors, with socioeconomic status allotted a lesser role, and rural-urban differences dismissed as unimportant once the other factors are considered. In Kleppner's version of the consensus interpretation, voting patterns emerged from a continuous and highly salient culture conflict between the "pietistic" (or "evangelical" or "low church") and the "ritualistic" (or "liturgical" or "high church") religious value systems. The pietists (Methodists, Quakers, Congregationalists, Scandinavian Lutherans, and most Baptists and Presbyterians), having created the Republican party as an activist vehicle for social reform in the 1850s, continued in the 1870s and 1880s to purify society of corruption—especially Romanism and Rum. The ritualists (Catholics and German Lutherans, especially) were the reformees. They reacted by supporting the laissez-faire Democrats. The religious cleavage was complicated by the ethnicity of immigrants and the differences among ex-Southerners, ex-Yankees, and ex-Yorkers, as Kleppner explains in great detail.

Kleppner analyzes both the documents and the voting patterns of key ethnocultural groups (controlling for economic level) to demonstrate the importance of the cleavages among religious outlooks. His focus is on the voter, with only a few narrative excursions into the schemes of politicians to exploit the possibilities. He effectively links voters with politicians by a highly original content analysis of the rhetorical thrust of newspaper editorials. He relies exclusively upon aggregate-level voting data, and when he introduces correlation coefficients (he never uses regression coefficients or multiple correlation), the reader becomes nervous about the ecological fallacy. Kleppner tries to avoid the fallacy by using homogeneous townships as much as possible, but then the question arises how typical were they? The problem is especially serious when he relies heavily on voting patterns from several highly erratic pietistic strongholds in southern Michigan for his interpretation of the election of 1896. Studies published since this book appeared have tried to avoid the ecological fallacy by using interview data from the 19th century—yes, immense quantities exist, in the form of directories and canvassers' record books.

Kleppner tries to be systematic—and he succeeds better than most historians—but he is not comprehensive. Contrary to the subtitle, the book focuses on the realignments of selected religious and economic groups in Wisconsin, Michigan, and Ohio from 1888 to 1896. Kleppner is successful in showing the importance of religious cleavages—though he ignores completely the large minority of Midwesterners without a denominational affiliation—and in gauging the impact of the 1893 depression. It is a nuisance, however, to have so much of the statistical evidence presented verbally by examples from supposedly typical townships, instead of neatly wrapped up in summary tables. Anyone trying to follow a specific group, or specific township over time would have to thumb repeatedly through the entire book. And the inadequate index will not help.

Social scientists who mistakenly believe that historians only tell stories will find Kleppner's style analytical, highly factual, and theoretically sophisticated. It is also clumsy, jargon-ridden and argumentative—sins easily forgiven in the face of great quantities of fresh new material. High-church readers will absolve the sins; evangelicals will crusade for stylistic reform; agnostics about history may be in for a revelation.

RICHARD JENSEN
University of Illinois, Chicago Circle

American Catholics and Social Reform: The New Deal Years. By David J. O'Brien. (New York: Oxford University Press, 1968. Pp. xi, 287. $6.50.)

The Catholic Church entered the Great Depression as a conservative institution but at the same time long concerned with social justice. American Catholics found their situation even more complex, for they had for almost a century been asserting the genuineness of their loyalties to both church and nation, and the election of 1928 had just revealed to them the continuing precariousness of their position. Moreover, Catholics in America were apprehensive with respect to government centralization and identified decentralization with their own interests and safety. Central to the American tradition was a strong liberal strand, but Catholicism and liberalism had long stood as opponents on significant issues. Socialism (and then communism) stood as almost a rival atheistic religion and a simplistic reaction to it tended to rigidify Catholic attitudes for decades. For a minority group still insecure in its closeness to immigrant beginnings, and to a religious body defensive in the face of modernity, these problems complicated what was in any case a difficult historical situation. Then came the Great Depression and the efforts by a Democratic administration supported by liberals and by the Left to do something about it, doing things that greatly increased the scope of federal government power. How in fact did American Catholics respond? Part of that answer—and a very diagnostic part—is provided by David J. O'Brien in this book.

The first response was positive. The new administration took more Catholics into government than had ever been taken before and Catholic writers on social policy almost unanimously supported the New Deal. Indeed Catholic University gave the new President an honorary degree before he was in office three months, and many thought, as did Father John A. O'Brien as late as 1937, that the New Deal was realizing the ideals of papal social teaching. Indeed there always remained strong rank-and-file Catholic support for the New Deal, a public opinion poll showed 66 percent Catholics voting for Roosevelt in 1936. But a great change was under way, and it was clearly heralded and reflected among the Catholic publicists and writers. Several events acted as catalysts. The attempt to pack the Supreme Court raised Catholic fears of centralized government. The alliance of liberals and leftists aroused the old fears of communism. The question of communist support for the New Deal arose in 1936 and remained a part of the picture from then on. The Spanish Civil War divided Catholics sharply from the majority of their non-Catholic fellows and exacerbated the "Red scare" among them. International developments in the latter half of the decade had similar effects. Catholics saw the drama of the church versus the revolution playing itself out once more and often responded not only with apprehension but with a simplistic and symbolic response that tended to skew their view of much else. Professor O'Brien traces out for us the developments that followed. We see the firm and consistent loyalty to the New Deal and to reform of Monsignor John A. Ryan and indeed of Cardinal Mundelein. We see the confused, confusing, demogogic and often popular performance of Father Charles E. Coughlin. We see Father Gillis move from support to opposition. We see the inner conflicts of *America* and *Commonweal.*

One of the most significant developments was the spectacular rise of Labor and especially of the CIO. While there was no general Catholic position on Labor, the labor movement had long been an important arena for Catholic participation in public life, and in this new situation it found many Catholic defenders. It also found critics. In fact, in spite of the strains tending to align Catholics with conservative forces, there is evident a striking hardiness of liberal Catholic attitudes.

Out of this there emerges a new clarity with respect to the significance of Papal social teaching in the period before John XXIII. All the writers examined claimed to base themselves upon the encyclicals of Leo XIII and Pius XI, yet the variety of doctrines presented testifies to the great gap between principle and reality—a gap Catholics never managed to bridge. The author also wisely treats of the Catholic Worker Movement, which, although a small element in the vast array of Catholic positions in the 1930s, significantly influenced American Catholic life in subtle ways. While this book is based upon the published statements of articulate and often influential Catholics and therefore does not tell us much about the variations to be found among American Catholics generally in the decade, it is a valuable work. The sample is diagnostic in many ways, and the basic strains and tensions inherent in Catholic life are revealed with care and objectivity.

THOMAS F. O'DEA
University of California, Santa Barbara

The Constitutional Polity. By Glendon Schubert. (Boston: Boston University Press, 1970. Pp. 195. $6.50.)

Ogden Nash has suggested that since one must sin, sins of commission are much to be preferred to sins of omission. Sometimes, though, lack of virtue has unexpected rewards. Had I not failed when asked to criticize the manuscript form of the lectures which are the basis of this volume I would not have reread to great profit this tighter effort and especially would not have worked my way through the new preface—which makes clear in a way the somewhat bland chapter headings do not—what the underlying premises of the volume really are. In that preface Schubert reveals a wish to develop "some rudiments of a behavioral model of constitutional change . . . a transactional view of the reciprocal relationship between American constitutional symbolism and political behavior, in application to a causal model of constitutional change." He notes that lack of the necessary preexisting data base and so settles for an even simpler treatment in terms of outputs ("Constitutional Policy"), attitudes ("Constitutional Politics"), and responses ("The American Polity").

Even at this compromised level there are severe difficulties, so that the three component lectures are less than happily parallel. As might be expected, considering the author's own work, the chapter on attitudes is strongest and fullest. The chapter on output is itself uneven, with the section on "Field's Court" rather routine, that on the New Deal refreshingly intense, and the more recent material something of a virtuoso exhibition of the range, energy, and penetration of mind Schubert can bring to bear on a subject. As he claims in his preface, he indeed does discuss policy outputs, not legal doctrine, and in a helpful way.

To my taste, however, Chapter 3 is something close to a failure. It is not parallel in temporal scope to the other two, perhaps because of lack of response data in the earlier period. But Schubert, so ingenious at finding surrogates for hard data in Chapter 1, sticks to the obvious handful of studies on popular and elite response even in more recent times. The need for psychological closure and for a sense of contemporaneity appropriate for the cathartic last lecture seems to overwhelm the analytic task, one which was perhaps even more difficult and significant than in the earlier two chapters.

So much for architectonics. In intellectual terms, it is interesting to note that this work is really much closer in tone to his earlier *Constitutional Politics* than, for example, to *The Judicial Mind.* This would seem to indicate Schubert's basic stance has changed little in the past decade and a half. His perfecting of attitudinal techniques appears to reflect a desire to sharpen research methods, rather than to substitute a paradigm of inquiry. To utilize Schubert's own terminology, this effort is an amalgam of "conventional" and "behavioral" approaches. These lectures suggest a greater affinity between his work and that of the process-oriented, nondoctrinal, but nonattitudinalist writers than some of his more pungent footnotes and aphoristic thrusts might recently have suggested. Some of us in the judicial process area have worried lest Schubert, like Herbert Simon, would reductionistically retreat into the recesses of individualistic psychology. This book is totally reassuring on that score. It is a thorough political analysis in which attitudinal studies enlighten, surprise, and refresh political perspectives.

My greatest quibble with the work is with what seems to me frequent cavalier use of data. Just because so much of the volume is meticulously reasoned and bristles with a remarkable range of data, the forays into sheer speculation seem especially out of place. Perhaps it is merely a question of proper labeling or inadequate footnoting where the case is established and I am not knowledgeable. But these seem to me to be among the more questionable items:

(1) At many points, transhistorical comparisons of voting records are made (e.g., John Clarke is labeled more liberal than Cardozo). I don't really disagree, but find it hard to imagine the empirical base on which this conclusion is reached.

(2) It is almost meaningless or simply wrong to suggest the Steel Seizure Case "could not have come at any other time than it did . . . with an outgoing presidency at the nadir of its authority" (p. 41). All historical events occur when they do.

(3) The statement that *Gault* "can only be regarded as a victory for majority sentiment in the organized legal profession" (p. 65) *contra* social scientist advice ignores the shift in sentiment in precisely those latter circles. *Gault* was at least as popular with liberals as conservatives, and Schubert's classification of it as otherwise raises questions about the objectivity problem in his schema.

A curious note is Schubert's effort to expand his classification of judicial liberalism and conservatism to encompass radicalism. The exercise seems prompted more by contact with departmental colleagues at Hawaii than by dealing with Supreme Court concerns. He suggests Douglas may possibly qualify as a radical; however, Douglas is negative on three of four criteria Schubert adumbrates, making the exercise even more curious. There are also several class-origin digs by which Schubert seems to be asserting his own proletarian bona fides at the expense of middle-class colleagues. These latter asides may of course have been fillips for the groundlings in a lecture series.

All in all, the work emerges as an interesting synthesis of several complementary traditions of constitutional analysis. It is not, in execution, a radically pioneering nor a wholly consistent effort, but it is one which sketches interesting vistas and which is self-evidently the product of great discipline, talent, and versatility.

SAMUEL KRISLOV

University of Minnesota

Kashmir Awakes. By B. L. Sharma (Delhi: Vikas Publications. 1971. Pp. 292. Rs. 28.50.)

Apart from being generally well written this book has very little to recommend it. Its sixteen chapters cover ancient cultural history, the rise and fall of monarchs, communal history, emergence of an independence party in the 1930s, partition and war over Kashmir, transfer of power from Maharaja to Constitution, the career of Sheikh Abdullah, economic and administrative developments since 1950, and recent official consideration of the advisability of changing Kashmir's constitutional status. The unifying threads in this rambling account are a thesis and a denunciation.

The thesis is that Kashmir has for centuries been remarkable for the amount of communal harmony among its diverse people. Episodes of religious bigotry are solely attributable to insensitive and fanatic rulers. Kashmir would be renowned for its dedication to the practice of secularism and religious toleration if only it were left alone by opportunistic politicians, and especially by outside—meaning Pakistani—agitators. The nature of its people demonstrates, therefore, its essential unity with India; the genius of India and Kashmir are identical. Any weakening of the political links between Kashmir and India is contrary to the fundamental nature of the Kashmiri people and represents a threat to secularism in all the subcontinent. Union with India and secularism in the subcontinent are indistinguishable.

The denunciation, which takes up two-thirds of the book, is directed against Sheikh Abdullah. Undoubtedly much that Sharma says about Abdullah is true—his vacillation, opportunism, weakness, and ambiguity. The only explanation, however, that Sharma can conceive for Abdullah's behavior since 1952 is that he is a tool of the rulers of Pakistan. In no other way can he account for Abdullah's strong support of secularism and accession of Kashmir to India between 1947 and 1952 as against his pressing of the issue of self-determination throughout the past decade. Abdullah is viewed throughout exclusively as a Muslim communalist, a turn-coat secularist. He is never viewed as a Kashmiri nationalist. Sharma's argument goes to absurd lengths. He charges Abdullah with colluding with Ayub Khan in infiltrating the

Valley in 1965. His evidence is that Abdullah visited Pakistan in May, where he must have become aware of the infiltration plans, because on his return he raised the issue of self-determination which created tensions in the valley, thus laying the groundwork for the popular uprising Pakistan counted on (p. 240). On the other hand, when Abdullah in 1968 took a stand in favor of a united India, including Kashmir, Sharma comments: "How could a man, so close to Pakistan, make a suggestion which is a nightmare to Pakistan leaders? Obviously there was more in it than met the eye" (p. 160).

The history in the book is rambling, selective, and unoriginal; analysis of issues and personalities is confused with partisan attack and diatribe. The book makes no substantive contribution. Because Sharma was Director for Kashmir affairs in the Ministry of External Affairs from 1956 to 1966, the book might have had value if it had been written as the memoir of a prominent actor. This opportunity, too, is missed. He makes no attempt to take the reader behind the scenes; he comments on what is essentially a newspaper record of events. The book, which simply offers another partisan perspective, is not worth reading.

DAVID H. BAYLEY

University of Denver

Military Justice is to Justice as Military Music is to Music. By Robert Sherrill. (New York: Harper Colophon Books, 1970. Pp. 240. $1.95.)

Can a journalist write for the scholarly trade? Anthony Lewis, who used to cover the Supreme Court for the *New York Times* had some success at it. Fred Graham, who replaced Lewis, is a less successful practitioner. At least the factual errors in his recent book, *The Self Inflicted Wound,* suggest some slippage in reliability. For Robert Sherrill, currently the Washington correspondent for *The Nation,* the answer may very well be, "No."

This is not due to any lack of mere writing experience. Sherrill has published frequently in such sources as *Playboy, The Antioch Review, Esquire, The New York Times Magazine,* and *Life.* He has also authored several books dealing with political themes. And, quite recently, he has written what his publisher calls a "unique new textbook for American Government classes." Sherrill, himself, is described as ". . . a man of strong opinions and a distinctive point of view . . . [who] takes a position on every issue, presents his views clearly and firmly, and backs them up with examples, quotes (many from personal interviews) and anecdotes." Horace Walpole would surely appreciate this find. For though unintended, the quotation aptly describes the author and the style of *Military Justice is to Justice as Military Music is to Music*—a book that is thought-provoking but partisan and polemical.

Basically, Sherrill argues that the military justice system and those who run it are bad, bad, bad! More than half his text is devoted to two chapters on "The Levy Affair" (in which Sherrill finds a "most fascinating similarity" to L'Affaire Dreyfus) and "The Presidio Mutiny." Throughout these detailed case studies and other chapters in the book, the same themes are pounded at the reader with bull-like thrusts. We are told that conditions in military prisons are "rotten"; that guards are brutal and psychopathic; and that torture of military prisoners runs the gamut from forcing a homosexual to "suck on a flashlight for the amusement of his Marine guards" to standing "naked in front of . . . mirrors . . . for twenty-one days." According to Sherrill, the Army has no respect for the Constitutional rights of servicemen, it keeps militants and antiwar GI's off balance, insecure, and rattled by frequent transfers, prosecutions, and other harrassments, and uses brain-washing techniques in its stockades.

As for the military court system, the adjectives are all negative. Sherrill perceives this system as one which degrades and exploits the serviceman. Juries are said to be stacked, commanding officers guilty of influencing verdicts, and due process a hollow phrase.

The particular facts, interviews, and anecdotes with which Sherrill makes his case cannot be checked for reliability, since the book contains neither footnoting nor bibliography. This is a particular problem since much of the book is based upon interviews with the twenty-seven "mutineers" at Presidio, various dissidents, and other military prisoners both within and without the United States.

Since it is a documented fact that in reporting their encounters with law enforcement officers, respondents often shade the truth, much of Sherrill's evidence is suspect. And much of it might have been embellished. At the same time, on the basis of my own research in this area and other validated cases, I am prepared to believe that much of the abusive behavior described in this book, or something similar, did occur. And since the material he assembles to make his point is a massive overkill, we could discount fifty per cent of what is reported without seriously undermining the basic complaint—i.e., that servicemen are needlessly restricted, abused, and denied basic human privileges.

When we move to inference regarding causes, motivations of individuals, and institutional policies, the case is less well made. That a stockade guard strikes a military prisoner proves neither that the guard is psychopathic nor that the Secretary of the Army ordered him to do so. Certainly, we cannot assume that Army officials consciously staff military prisons with psychopaths.

In his sixth chapter (p. 213), Sherrill says: "It would be wrong to conclude that the military establishment has a higher percentage of sadists than any other element of the population, or that the military establishment would not prefer to bend and break its personnel under polished and sanitary conditions. Nor should one conclude that most military commanders knowingly encourage brig and stockade officers and non-coms to engage in the downright cruel bullying that goes on." But the entire book seems designed to leave one with precisely that impression. And Sherrill concludes the above quotation by saying, of course ". . . the highest military officers . . . would doubtless prefer to achieve total discipline with a minimum of blood and abuse." But only as a matter of "good public relations." While such a statement may or may not be valid, it is certainly not established by this book.

Sherrill's perspective on the military court system is similarly jaundiced. While he correctly points to such continuing weaknesses as "command control," he neglects to give equal time to more favorable aspects of the system. As I have reported elsewhere, for example, the serviceman's right to counsel is, in a number of respects, superior to that of his civilian counterpart in state and federal courts. Several of the rights established for the criminal suspect in *Miranda v. Arizona* were available in the military justice system some fifteen years earlier. And the recent acquittals of Col. Oran Henderson and Capt. Ernest Medina show, inter alia, that military courts are capable of resisting major governmental efforts for a conviction, involving massive expenditures, and a profusion of legal-factual complexities that would tax most civilian courts. In sum, the picture, though certainly in need of improvement, is not as bleak as Sherrill paints it.

A capsule verdict? Polished muckraking!

S. SIDNEY ULMER

University of Kentucky

The Making of the Israeli Army. By Yigal Allon. (New York: Universe, 1970. Pp. 273. $8.95.)

Zanek!: A Chronicle of the Israeli Air Force. By William Stevenson. (New York: Viking, 1971. Pp. 352. $8.50.)

The Seventh Day. Edited by Avraham Shapira. (New York: Scribners, 1971. Pp. 276. $6.95.)

Currently the Jewish stereotype is undergoing a dramatic change. The parasitic model, which still scores well among anti-Semitic, and anti-Zionist and anti-Israel forces seeking the traditional scapegoat to world problems, is being challenged by the young Israeli Defense Force recruit, a member of what has become one of the most efficient and daring military units in world history. Like all national stereotypes, these are absurd generalities and gross mythologies derived from some semblance of fact but abstracted from context and distorted to fit into a preconceived propaganda frame of reference. What has been missing is a more balanced picture of the changing Jewish ethos, seen in historical context and illustrated by case studies of actual individuals who shape and are shaped by the drama of contemporary Jewish history.

These three books, in their different ways, provide something less than an adequate survey but much more than has been available to date. They attempt to contrast the Jewish stereotypes, to sift fact from fiction, to describe the effort to change the Jewish image, and to account for dysfunctional consequences both within Israel and on the international propaganda front. The rather formal presentation of Perlmutter's *Military and Politics in Israel* (Praeger, 1969) is dropped in these books for analytical survey, behavioral insight, and anecdotal illustration. The Deputy Prime Minister of Israel and former army hero, Yigal Allon, has contributed an extended essay concerned with the Israeli Government's military philosophy, policy, and strategy rather than an administrative and technical history as the title *The Making of the Israeli Army* might imply; nevertheless, half the book contains several important reports and policy statements, not easily available in English, describing military actions undertaken between 1941 and 1967. Allon could write a definitive history, for he was an important contributor to the development of the Israeli Defense Forces, but he has limited himself to a rather breathtaking sweep in which hardly a word is wasted. This is a very competent official version, though critical in places, but skirts some of the major controversies such as the role of irregular groups, the forcible disbanding of Allon's Palmach and the inability to convert military prowess into international political standing. What it does, it does well, but it does not do enough. Perhaps much of the Palmach documentary section could have been sacrificed for more of Allon's personal reflections on the events as they unfolded around him. That would be a book to match Dayan's Sinai diary.

Stevenson's *Zanek!* the title taken from the Hebrew equivalent for "scramble" is an overwritten personal account of the daily operations of the Israeli Air Force during the "war of attrition," 1969–70. It is splattered with anecdotes and personal reflections, interspersed with sharp digs at Israel's critics, emotional justifications of Israeli attitudes, and a first-rate account of the history and administration of the Israeli Air Force. Where Allon is modest about Israeli achievements, Stevenson, a non-Jew, is boastful. His book is good copy for the Jew-as-hero theme most popularized in the film *Exodus*. American Zionists will love it. Israelis will blush. Anti-Zionists will find it yet another example of Zionist dross and propaganda. Yet there are nuggets that are worth the labor of skimming, particularly those parts dealing with Israeli air tactics, pilot morale, technical proficiency and political confines of military action, which go as far as military censorship permits.

The Seventh Day is a dateless classic originally produced for private circulation but edited into an Israeli bestseller that should have worldwide appeal. It consists of a series of discussions conducted by selected interviewers with different groups of kibbutzniks who fought in the Six-Day War. Little of the original flavor has been lost through transcription and translation. It is frank, unguarded, emotional, and real, a universal reflection of how the common soldier feels about war, fighting, death, destruction, re-echoing the sentiments of *All Quiet on the Western Front*. Allon's political strategic analysis turns into the hopes, fears, ambitions, regrets, promises, doubts, worries, escapisms of individual soldiers and citizens who somehow fit into the grand design. Shapira's heroes are mortal sophisticated people who hate war but reluctantly find themselves proficient at it as the only way they see of guaranteeing their own physical survival, of protecting the product of their love and labor, of ensuring a meaningful inheritance for their children, and, no less significant, of preserving the historical links between Jews and their ancient homeland. Reviewing contemporary history and understanding international power politics, they believe they have no alternative but wish it were otherwise. Philosophically resigned to "what will be, will be," they add a plaintive prayer that it may not all be in vain.

The contrasting stereotypes illustrate that history is written by the victors. The parasitic image was concocted by non-Jews seeking justification for genocide or forcible conversion and assimilation. What the Jews were really like nobody really knows because they left no trace. The vanquished are only remembered by the praises heaped on them afterward by those who wiped them off the face of the earth. The six million dead of the Holocaust are not quite forgotten yet by non-Jews but are rapidly disappearing into a history no one wants to recall readily. The Nazis claim they deserved their fate because they went willingly to their deaths like sheep, and anyway the numbers are highly exaggerated. They conveniently overlook the fact that most of their victims were young children, senile grandparents, starved women, diseased men, isolated, deserted, exposed, cheated, betrayed, and unlamented. They downgrade the ghetto resistance, the death-camp revolts, the partisan groups and the exodus to the east to join with the Soviets. Recovering from the shock of the Holocaust, the Jew is rediscovering himself in the glory of the Israeli Defense Force. He remembers the Diaspora Jew as a fighter not only in defense of his own, but also as mercenary, professional, volunteer, servant of his rulers; not only in self preservation, but also as martyr for religious freedom, civil rights, social equality, political liberty, economic opportunity, and cultural autonomy. Like the *sabras* in *The Seventh Day*, he worries about the contradictions between Jewish ethical teachings and the reality of the Jewish predicament. The warrior image repels. Equally, the role of righteous martyr is unattractive. So he finds himself as reluctant warrior and reluctant hero, longing for the end of his troubles. Just as the Diaspora Jew wants a quiet life, so does the *sabra*, who still clings to the original Zionist dream that he should be welcomed by local peoples, greeted as an old friend returning home, treated as a partner, offering his labor, skills and enterprise in return for peaceful cooperation, mutual tolerance, and common development, not as an intruder set on subjugating the Middle East in pursuit of grandiose designs to master the world. As long as his own self-image is not accepted by others, he is willing to pay the price in keeping one step ahead of the other guy, so vividly described by Allon and Stevenson, and intoned by the *sa-*

bras of *The Seventh Day*. All told, some fascinating reading is found in these three books, indispensable for students of the Middle East and of newly emerging nations.

GERALD E. CAIDEN

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The Risorgimento and the Unification of Italy. By Derek Beales. (New York: Barnes and Noble, Inc. 1971. Pp. 176. $7.50.)

Had the author of this slender volume, which is part of a British series of *Historical Problems—Studies and Documents* intended for college undergraduates, contented himself with writing a competent survey of his subject, he would most likely have succeeded. But Beales, who is capable of acute insights and is familiar with the latest writings on the Risorgimento and Italian unification, set himself the higher goal of challenging the "traditional" and "patriotic" schools of Italian historiography. On the whole, he has failed in his endeavor.

On p. 14 of the book, which is divided into two almost equal parts consisting of a historical sketch and an anthology of documents, Beales states quite rightly that the precise relation between Italy's national revival (the Risorgimento) and the process of unification is exceedingly hard to determine. He then declares somewhat dramatically that, "merely by posing this problem I separate myself from the main tradition of Italian historical writing," emphasizing that the actual unification of Italy, almost completed in two years (1859–1860), was "the sudden and accidental upshot of war and diplomacy," and not a result or stage (as maintained by most Italian historians) of the national revival originating in the eighteenth century.

Beales's position resembles somewhat that of his teacher and mentor, Denis Mack Smith, who, however controversial in some of his opinions on modern Italian history, usually has the merit of developing them to their logical conclusions with abundant if not always convincing documentation. The disciple is not up to the achievements of his master; for although Beales is free with caustic remarks regarding the intimate linkage between Risorgimento and unification propounded by such diverse writers as Giorgio Candeloro, Benedetto Croce, Niccolò Rodolico, Luigi Salvatorelli, Antonio Gramsci, and others, his own narrative survey hardly differs in any substantial way from the much lengthier works of the Italian and Italo-American historians he has set out to refute. In explanation of the "problem" he has posed, Beales states that "an English historian . . . cannot, in the first place, fully share the patriotic feelings of Italians. And, more serious, his philosophical presuppositions will be different from those of most Italian historians" (p. 14). But what Italian historian of any merit has ever denied altogether the importance of chance, and of war and diplomacy, in the story of Italian unification? At most, whatever there is that could be called original in Beales's work is the emphasis he places on the role of accidental results.

As a concluding criticism, it must be noted that the title "Introduction" which the author chose for the whole first part of his work suggests that the narrative account is intended to serve, at least in part, as background to the sixteen documents located in the second portion of the book. Even this expectation is disappointed. There are frequent notations in the "Introduction" referring the reader to pertinent documents, but the latter are rarely discussed at any length in the narrative itself and are not even given the briefest commentary in the documentary section of the book. If Beales's book is used as a vehicle for classroom instruction, this omission can be corrected readily by the instructor who, interrupting the account of events at appropriate times, can examine and relate the documents to the relevant portions of the narrative. But the general reader, as distinguished from the classroom student, has no such academic guidance and is obliged to make the best use he can of what are indeed judiciously selected and useful documents. The value of these documents is thus correspondingly diminished in this uneven and disappointing work from an author whose earlier studies in Italian history promised something more impressive than what we have been given in the book under review.

SALVATORE SALADINO

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Peru 1965: Notes on a Guerrilla Experience. By Héctor Béjar (New York and London: Monthly Review Press, 1970. Pp. 142. $6.00.)

This short book provides us with an excellent translation (by William Rose) of Héctor Béjar's account of some of the episodes related to the 1965 guerrilla attempts to gain power in Peru. This book, first published in 1969 after it won the Casa de Las Américas essay prize for that year, has been read widely throughout Latin America and has been translated into other languages. The author, a member of the Peruvian Communist Party since his youth, broke with it in the late 1950s over the party's preference for an electoral strategy to gain power. A few years later, Mr. Béjar and other former young Communists organized the Army of National Liberation (ELN), for the specific purpose of overthrowing those then in power through the use of force. Finally, in 1965, the Army of National Liberation contributed one of the three guerrilla groups that almost simultaneously went into action in the Peruvian highlands. Mr. Béjar, who was known as a writer prior to his guerrilla experience, commanded this group. Thus, this is the account of a leading actor in an event whose long-range consequences may never be properly assessed, regardless of its short-range failure.

When everything is taken into account, however, the book disappoints me. It disappointed me in 1969, when I first read it in Spanish, and that disappointment remains after reading the English version. For the fact is that Mr. Béjar says very little that has not been publicly said before, in such solid works as Rogger Mercado, *Las Guerrillas en el Perú; el MIR: De la Prédica Ideológica a la Acción Armada* (Lima: Fondo de Cultura Popular, 1967); Silvestre Conduruna, *Las Experiencias de la Última Etapa de las Luchas Revolucionarias en el Perú* (Lima: Editorial Vanguardia Revolucionaria, 1966); Victor Villanueva, *Hugo Blanco y la Rebelión Campesina* (Lima: Editorial Juan Mejía Baca, 1967); Américo Pumaruna, "Perú: Revolución: Insurrección: Guerrillas," *Cuadernos de Ruedo Ibérico,* April-May, 1966, pp. 62–86: or the more sensationalist Gonzalo Añi Castillo, *Historia Secreta de las Guerrillas* (Lima: Ediciones "Más Allá," 1967.)

Béjar discusses the causes that led to the creation of the Army of National Liberation and the guerrilla actions in which he was involved, and he reviews their errors and possible recurrence. As indicated above, many of these points had already been developed competently by other writers. On subjects such as the errors committed by the guerrillas, the divisions and lack of coordination between the various subversive organizations, and the amount and depth of popular support received from the peasantry, which he is in a position to discuss with authority, the author appears either confused or reluctant to be totally truthful. His repeated references to the communication gap between the urban middle-class guerrillas and the peasantry blame such formal elements as the language barrier: most peasants of the area in which his group was operating preferred to speak Quechua, while only a few guerrillas spoke it, and then not even the local dialect. This emphasis seems a weak excuse; all available information shows that there was much more than lack of communication in the guerrillas' failure. Their inability to foresee the severity of the repression (p. 110), and their lack of adequate clothing (p. 97) only prove that this was an amateur operation which misinterpreted Peruvian reality, and not only because of "insufficient and interrupted theoretical work" as Béjar concludes on p. 126.

Béjar does not clearly face the question of peasant support: on pp. 96, 99, and 100, he speaks of an enthusiastic reception and the development of close ties with the local inhabitants. On p. 105 he begins to distinguish between different meanings of the expression "peasant support"; on p. 107 he indicates that the dependency on local recruitment of guerrillas was an error; finally, on pp. 114–18, he recognizes that the guerrillas' relationship with the peasants had not been very close, owing to lack of time and the remoteness of the guerrillas' thinking from peasant reality. The fact of the matter is that neither the guerrillas nor the government forces had the support of the peasantry. The rural lower class did not trust these alien factions, led by members of modern urban Peru and engaged in a struggle for power among themselves. The Indians and mestizos simply went along with whoever was in control of the immediate area. In all fairness to Béjar, he intimates this much, but is unwilling to discuss it openly.

The 1965 guerrilla actions in Peru had a profound effect on the nation's politics. Many military officers were encouraged to become a developmental elite (I am not arguing here whether they have achieved that objective) by their counterinsurgency operations. The difficulties they faced, particularly in their struggle against the group effectively led by Guillermo Lobatón, made the officers think about the possibility of future, better organized, guerrilla movements, and about what might happen to the military establishment were they to succeed. Even in victory, many officers apparently disliked their counterinsurgency role and felt that the best policy was *to prevent insurgency, rather than to repress it.* The counterinsurgency campaign also exacerbated and brought into the open existing disagreements with American military advisors and led Peruvian officers to believe that the existing level of American penetration was against the Peruvians' best interest. Many of the actions taken by the military regime which has been governing Peru since October 1968 can be traced back to the antiguerrilla campaign of 1965. At any rate, many of the military officers who have occupied important political positions in the present regime were involved in the 1965 operations.

In view of Mr. Béjar's conclusions that "armed struggle is the only road left for liberating Latin America" and his promise to "continue the road we have undertaken with firmness and zeal" (pp. 127–28), this book has a curious sequel: the author, together with other guerrillas, was released from prison by the military government on December 22, 1970; since then, he has joined the staff of progovernment publications and of SINAMOS, a government agency whose objective is to mobilize lower class support for the military regime.

In closing, I should like to congratulate Mr. Rose and the Monthly Review Press for making this type of material available in English; at the same time, one wonders why it was chosen over far superior contributions, such as the books by Mercado and Villanueva, mentioned earlier. Let us hope that they will now make available other accounts of this important political event.

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A History of Brazil. By E. Bradford Burns. (New York and London: Columbia University Press, 1970. Pp. 449. $11.95.)

Brazilians say, tongue in cheek, that to write about the history of our country we have to do research in the United States. The remark may seem mischievous, but the truth is that recently more has been published on an academic level about Brazil in English than in Portuguese, and the specialized libraries in the United States are undoubtedly rich in the documentation a researcher needs. In 1966, in surveying American Ph.D. dissertations and research themes currently focusing on Brazilian history and social sciences, I discovered that there were more than a hundred of them—far more than were being written by Brazilian students at the time. The present massive production is the result of a long cumulative process started during the 'thirties, when the United States was beginning to be a superpower. Brazil was then ceasing to be merely a *country of the future* and was becoming a laboratory of historical experiences challenging the imagination of social scientists. Charles Wagley and Stanley Stein, to speak of only two of the foremost pioneers, paved the way and established a basis for the following generations.

Bradford Burns's *A History of Brazil* is undoubtedly one of the most remarkable of the numerous recent books on Brazil. Drawing on an excellent bibliography and well-handled primary sources, Burns presents the political evolution of Brazil and offers a comprehensive picture of the economic, social, and cultural development of the country. His approach is that of a scholar familiar with varied sources—historical, anthropological and literary. But it is also that of an essayist trying to communicate his faith in the destiny of a nation to which his sympathy flows. In this sense *A History of Brazil*, without being exactly a textbook for college students (a *manuel* in the French sense), is something of a feat in being the first scholarly book on Brazilian history from colonial times to this day.

This text is intended to be a synthesis for academic and nonacademic use. It follows a methodological line familiar to Anglo-Saxon historians which presumes that history explains itself largely through the testimonies of its contemporaries—travelers, chroniclers, and participants in general. Along these guidelines, the historian's role is not to go beyond the accounts he collects from his witnesses. He minimizes the discussion of historiographic tendencies and theoretical explanations, both sociological and economic. Likewise he disregards the more modern methodological concerns about demography, economy, and social classes of the world-famous French *École des Annales*. Herein lies the principal weakness in Burns's approach: its rather conservative methodology. In this way, it may be disappointing to more sophisticated Latin American and European students.

On one hand Burns's book has the exceptional value of being a global presentation of Brazilian history and thus is a manual of great help for historians, social scientists, literature specialists, and readers interested in Brazil. But on the other hand it limits itself to presenting the political, economic and social evolution of an internal Brazilian history without taking into account the colonial phenomenon that conditioned it. A better analysis of colonial society and economic system would help us to understand more clearly the power structure that developed since the country's political independence in 1822. This society was based upon an intrinsically interwoven *latifundium*, monoculture and slave system. It was oriented towards foreign markets and geared to a cyclical and predatory economy resulting from the mercantile structure of the plantation system. In the same way one historical process and only one is constituted by the specific formation in Brazil or a peculiar type of capitalism, the up-and-down trends of Brazilian democracy, military interventions in the country's political life, and the extreme poverty of most of its population in contrast to the wealth of a few. All these must be viewed as integral parts of the European and North American capitalist development. I am not one of those who favor a mechanist explanation of history. But there certainly is a dialectical interplay between the external and internal factors that formed Brazilian society from the first colonial days to our present capitalist development and military dictatorship. This is not to be confused with, or mistaken for, stereotyped generalizations.

Bradford Burns is undoubtedly a competent historian. His knowledge of Brazil, its people, and its cultural expressions is indisputable. His work is commendably earnest; his documentation is outstanding. He is limited, however, by the method he employs to attempt an explanation of Brazil, for he forgets that the history he describes belongs to a larger context. And though it may seem paradoxical, he appears to carry unconsciously within himself a North-American historical model in which the middle classes are held to be the agents of a given democratic ideal, of certain political, economic, and ideological values; Burns searches for the equivalents of these values in a different historical environment—that of Brazil. Does he find them? According to the author, there is, for instance, a difference on the one hand between the liberal (*moderadores*) and modernizing military who came to the fore in 1889 and also from the 'twenties to the 'fifties, and on the other hand the anticommunist military of 1964 and 1968. The author emphasizes this difference rightly. But he does not explain what made this behavioral pattern change. And when he says in his last paragraph that "many of the leaders who came to the fore after the coups of March 1964 and December 1968 seem turned to the past," one is likely to ask, among other things, whether his interpretation is correct. Instead of saying that the military today are tied to "the conservative, oligarchical, and patternalistic patterns of the past," would it not be more correct to acknowledge the view according to which the present military dictatorship leads the modernization process of a peculiar type of capitalism in Brazil?

A history of Brazil that fails to embrace in its explanation the determining external factors, chronologically centered on Portugal, Great Britain, and the United States, as well as the class structure that controls the domestic system of political domination will always be incomplete. To be complete it should incorporate as well the theoretical contribution of contemporary Latin American and European economists and social scientists. I admit that such a history of Brazil has yet to be written.

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The Net Cost of Soviet Foreign Aid. By James Richard Carter. (New York: Praeger, 1971. Pp. 134. $12.50.)

In recent years there have been several attempts to estimate the real cost or net cost of foreign aid (which is characteristically less than the nominal or apparent cost) from the donor nation's viewpoint. Most of these works have limited themselves to estimating the true subsidy or grant element involved in official foreign aid programs—in other words the real economic costs of foreign aid for the donor. These studies have typically considered the real costs to be composed of two elements: grants plus loans, less the sum of loan repayments discounted over time at an interest rate that would reflect the return that the lender might have earned through alternative employment of the loaned funds. There have also been a few attempts to account for the fact that most bilateral aid to underdeveloped countries is "tied"—offered on condition that the recipient use the aid only for purchases in the donor country—and therefore may cost the donor less than the nominal price, further reducing the donor's real costs. (This would be the more likely in the case of socialist donor states, where prices in international trade are less likely to reflect real costs than in market economies.)

Carter, in his monograph, has not only carried out these tasks, which are particularly difficult for the USSR in view of the paucity and uncertainties of the data, but has also attempted to estimate two other factors which tend to reduce the real costs of aid: the gains to the Soviet Union resulting from increased trade with those countries that received Soviet aid; and the degree to which prices paid by the USSR for imports from less-developed countries were lower than comparable world market prices. As a consequence of these calculations, Carter estimates that the real cost of Soviet foreign aid during the period 1955–1968 was $681 million, as compared to the nominal or apparent level of $6300 million.

The task that Carter set himself is difficult for several reasons. For example, there are major data problems; the analysis raises several technical economic issues which are not easy to resolve; and it raises issues of values, both economic and political, which may be inherently insoluble.

First, as to data: Soviet data on actual aid deliveries (as distinguished from promises of aid) are sketchy, and estimates of recipients' loan repayment terms and conditions, as well as of overpricing of tied aid and underpricing of Soviet imports from less-developed countries are also highly uncertain. Thus, Carter estimates that from 1955 to 1968 the Soviet Union probably paid from 10 to 15 per cent less on average for its imports from poor countries than it would have at world market prices (p. 41). In fact, as he freely admits in the text, such estimates mean very little. Hindrances to accuracy include quality differences between Soviet imports and world market average qualities and lack of accurate pricing information on Soviet imports. Nonetheless in estimating the real cost of Soviet aid (Table 4), he assumes that the USSR in effect received a 15 per cent discount on its purchases, with an equivalent estimated reduction in the real cost of aid.

Second, as to the economic analysis: Some important points in Carter's analysis are open to discussion on technical grounds. For example, there is a distinction between donor's real cost and recipient's real benefit. That is, while the USSR *may* have benefited by paying less than world market prices for coffee during the period 1955–1968, there may also have been excellent and profitable reasons for coffee-exporting nations to sell at lower prices to the USSR. First of all, under the provisions of the International Coffee Agreement, there is an export quota set for each coffee-exporting country. That quota does not apply to so called "new markets," including the USSR, where relatively little coffee is consumed per capita. Therefore, the real alternative for coffee-exporting nations in disposing of their surplus production is not the world market price, but whatever price they can get in "new market" areas, including the USSR. The equilibrium price in the new market would clearly be below world market prices as established *de facto* by the International Coffee Agreement because coffee-producing nations have been generally ineffective in their efforts to control production.

More generally, where formal commodity agreements do not affect market structure, to the extent that the USSR is a new market for tropical products or other exports of poor countries, then price concessions by less-developed countries may be a rational expression of self-interest. If price cutting helps them build up a new market in the Soviet Union, their long-run economic interests may well be maximized.

Another point arises in Carter's laudable effort to estimate the effects of Soviet aid on its foreign trade, notably with aid recipients. He asserts (pp. 64–66) that this effect can be determined if Soviet trading partners can be divided into groups of countries which did or did not receive aid and are otherwise basically similar. He says (p. 66) that by definition the only basic difference between the two groups of less-developed countries (Soviet aid recipients and nonrecipients) is that one group did receive aid while the other did not. While this proposition may be true, it is not necessarily true. During the period 1955–1968 nearly two-thirds of Soviet aid to non-Communist poor nations went to four countries—India, U.A.R., Afghanistan, and Iran. It is not clear that these countries are otherwise similar to other poor countries not receiving Soviet aid, even in the limited sense of their probable trade response in the presence or absence of foreign aid. Therefore it is equally unclear whether, "The difference between this estimated value of trade with aid recipients [in the absence of Soviet aid] and the actual amount of Soviet trade with aid recipients will constitute the estimate of the value of Soviet foreign trade generated by the Soviet economic aid program . . ." (p. 66).

Another question of economic analysis is the appropriate interest rate by which to discount the stream of repayments that the Soviet Union receives over time from aid recipients (pp. 28–31). Carter correctly states that the appropriate discount rate for this purpose is that which reflects the social rate of return on capital in the USSR. He chooses on that basis a discount rate of 15 per cent. But, the two pieces of evidence he cites (p. 31) reflect the rate of return to industrial capital. The actual social marginal rate of return to capital in the USSR may well be lower or higher, essentially because of the absence of a market system for lending and borrowing capital. Therefore all estimates are essentially conjectural.

Third, as to issues of values: Carter asserts, "The ultimate goal of world domination is an inherent part of the communist doctrine . . . the evolution of Soviet foreign trade and economic aid policies and programs, since they are part of Soviet foreign policy, coincide in many ways with the evolution of this same foreign policy toward the less-developed countries of the world" (p. 3). But as he himself points out later (p. 7), contemporary Soviet foreign policy toward less-developed countries is only distantly related to aims of world domination. It currently appeals primarily to sentiments favoring national independence, economic development, and a peaceful world environment (with evident exceptions where vital Soviet interests or tempting targets of opportunity arise). As I have pointed out elsewhere, it is not clear that there is much to be gained by great powers today from winning the friendship of less-developed countries. For that matter, it is not even clear whether, from the Soviet Union's viewpoint, communist domination of such countries as China, Albania, Yugoslavia, or even Cuba represents a net political advantage. The motives for foreign aid remain mixed, obscure, and therefore susceptible of a variety of rationales, none of them wholly convincing to the general public in donor countries, which is a major reason for the modest level of support now accorded less-developed countries by capitalist and communist nations alike.

JOHN PINCUS

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Armed Struggle in Africa: With the Guerrillas in "Portuguese" Guinea. By Gérard Chaliand. (New York and London: Monthly Review Press, 1969. Pp. xvi, 142. $5.50.)

Chaliand's book combines two documents of interest to the political scientist. The first is a *maquis* diary recounting his impressions of a two-week tour of the guerrilla-controlled areas of the Portuguese colony of Guinea-Bissau. The second is a short essay on the "objective conditions of the armed struggle" in Africa, written from a radical Marxist perspective. A French *journaliste engagé,* Chaliand was attracted to the Guinean liberation movement as a possible model of revolutionary change in black Africa.

His travel notebook is valuable as one of the few "behind-the-lines" reports of the military situation in the colony. The central concern of his account, however, is to present the ideology of the political leadership. It is "the Party" which emerges from the narrative as the crucial instrument of political transformation, not merely from colonialism to independence but from traditional values to a socialist society. The Party in question is Amilcar Cabral's PAIGC (African Independence Party of Guinea and the Cape Verde Islands). Chaliand devotes long passages to Cabral's speeches in the guerrilla camps in order to illustrate the party's effort to forge revolutionary political consciousness out of the experience of armed resistance. Mere independence is insufficient according to Cabral: "It is time to put an end to colonialism, but we must also put an end to the backwardness of our people. . . . If we want respect, if we want to raise our heads, we must follow the Party. . . . My message is 'Work, and confidence in the Party!' " (pp. 33, 93, 70). While Cabral's notion of the primacy of ideology is not unfamiliar, the originality of Chaliand's work is to present these ideas in a real social setting: the Guinean *maquis* as a workshop in the application of political theory.

The author's purpose is to further the goal of radical change in Africa by demonstrating the achievements of the PAIGC and enlisting sympathy for its cause. Chaliand reports progress in two realms.: the concrete social advances constituted by the schools, clinics, and village committees established in the liberated territories, and the intangible ideological progress represented by the new attitudes expressed by the participants in the struggle. While Chaliand recognizes that many Guineans remain loyal to traditional values, he is clearly optimistic that the Guinean combination of well-articulated theory (PAIGC) and committed action (armed struggle) does hold promise for radical change.

Chaliand's essay on the broader outlook is less sanguine. Africa is not yet ripe for social revolution. He attributes the continuing grip of neocolonialism in Africa to several conditions: agrarian reform is not a pressing issue for the peasantry; the level of productive forces remains low; and the middle class has already acquired power, i.e., "the bureaucratic bourgeoisie is more numerous than the proletariat" (p. 115). These factors lead Chaliand to challenge the relevance of Che Guevara's *foco* theory for Africa. Rather than a guerrilla movement serving as a focus for insurrection, extensive political work to identify and mobilize the sectors of the population most sympathetic to radical methods must precede armed challenges to the existing regimes. The specific here intersects the general, for this is the strategy that was pursued by the PAIGC. Not all readers will share Chaliand's conclusion that the Guinean revolution has therefore been able "to capture the rank of vanguard" (p. 125), but many should find the analysis provocative.

Chaliand does not fully engage the question of whether a strategy applicable to the colonial context is equally applicable to the postcolonial context. Furthermore, the five years that have elapsed since publication of the French edition have shown that early successes do not insure rapid progress. But neither immediate victory nor vanguard status is required to make the Guinean experience interesting as a study in the redefinition of political values in contemporary Africa.

The English version is careless at a few points—most notably when "Niger" becomes "Nigeria"—but it captures well Chaliand's style of *reportage engagé* which conveys a lively sense of having shared a moment in the life of a guerrilla movement.

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Singapore: The Politics of Survival, 1965–1967. By Chan Heng Chee. (New York: Oxford University Press, 1971. Pp. 65. $3.75.)

This brief monograph is an amended version of the author's Master's thesis submitted to the Department of Government at Cornell University in 1967. Its approach is an essentially traditional chronological coverage of the subject matter, although the work contains intermittent low-level analysis and commentary. It is a highly generalized study, yet it presents a thorough and comprehensive overview of the internal and external forces which have shaped the emergence of this new Southeast Asian city-state whose political independence is less than a decade old.

The initial portion of the book presents a description of the political, economic, and military factors which led to the merger of Singapore and Malaysia in 1963, as well as the divisive forces which resulted in the failure of this experiment in federation only two years later. Thereafter the narrative focuses primarily on the key issue of "survival" for the 2,000,000 Singaporeans comprising a vigorous Chinese majority strategically located between large and suspicious Malay societies in Malaysia and Indonesia. According to the author: "Singapore's survival as an independent state is predicated on her ability to come to terms with her environment (p. 11.)"

In the two years covered by this study the People's Action Party (PAP) led by Prime Minister Lee Kuan Yew made considerable progress toward this goal. The political leaders in Singapore promoted rapid economic development and sought to win the cooperation of the small Malay minority residing on the island. They quashed the internal political threat of the extreme leftist Barisan Sosialis and dissident students by an effective mixture of persuasion and suppression.

In the external realm, survival has been bolstered by seeking a diminution of postseparation hostility in Malaysia and by establishing mutual trade and defense agreements with the political leaders in Kuala Lumpur. The withdrawal of British military power from Southeast Asia has required even closer security consultations between the ruling elites in both countries and a common effort to elicit some form of informal support from the United States. In addition, Lee Kuan Yew has negotiated trade pacts with the Soviet Union and Eastern European countries to retain some facade of "non-alignment." He has also supported moves toward Asian regional cooperation.

The study concludes with a description of the PAP's "Ideology of Survival," which in some respects might be considered a noncommunist Chinese riposte to the "Quotations of Chairman Mao Tse-tung." The three principles of the Singaporean ideology include the formation of a "new man" in a multiracial island society which can serve as an example to all of Southeast Asia, the formation of a "tightly organized society" characterized by resolution, discipline, and determination; and the inculcation of the virtue of "public-spiritedness" designed to extend personal loyalty beyond the family to the process of nation building. This doctrinal admixture has promoted unity and consolidation within the emerging island society; it has also weakened cooperative relations with the more relaxed and easygoing society in Malaysia.

The book is compact and well written. It is scholarly and objective. It shows much careful research and extensive personal knowledge of Singapore's politics. It provides numerous useful insights into the early history of this independent island republic.

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The Conquest of Violence: Order and Liberty in Britain. By T. A. Critchley. (New York: Schocken Books, Inc., 1970. Pp. 240. $8.50.)

This book is notable not only for its author but also for its scholarship. Mr. Critchley is an Assistant Secretary to the Home Office, and is therefore writing about matters which come within the professional concern of his administration, a most unusual departure from normal British civil service ethics. What emerges of the author through his scholarship, if he faithfully reflects in his person his administration, is a special mixture of concern, toughness, and humanity which tells us a great deal about the attitudes and preconceptions of those responsible for the maintenance of law and order in Britain.

It is, however, important at a very early stage to realize that Mr. Critchley is concerned with England and not with Britain. There are far wider issues which would have to be faced if a historical study were to attempt to relate the English methods of pacification in Scotland and Ireland. But in the narrower context of England, this is a most sophisticated and thoughtful piece of historical work, tracing the emergence of the institutions for maintaining the peace from the times of Anglo-Saxon England to the most recent developments of 1968. Mr. Critchley uncovers some fascinating themes, showing how, until the 1820s there remained in a type of collective memory, principles of equity and freedom which stem from Anglo-Saxon times and survived, despite the ravages of the Norman Conquest, in the hearts of oppressed people as remembrances of the good and ancient law. Or again there is the intriguing notion of a consistent pattern in English mob behavior—orderly, with set and limited objectives, intent upon issuing a warning of distress by destroying specified targets known to all as the instruments or the home of the oppressor. Also illustrative of this theme is the realization of the model police forces, set up during the nineteenth century, that demonstrations being legitimate, need to be controlled not by violence but by compromise and collaboration worked out in advance between the police authority and the leaders of the demonstration to reduce the dangers of confrontation, escalation, the unforeseen, and the unpredictable.

What, however, in the current setting is absorbing, is that despite all the legends to the contrary, the English far from being traditionalists, consistently manage to forget the lessons of the past. Throughout Mr. Critchley's account it is clear that disaster and death followed whenever the government called out the militia and the army to quell disorders. The authorities' most potent successes occurred when they had advanced knowledge of impending troubles through their intelligence service, and managed to nip uprisings in the bud. Yet, even after Sir Robert Peel created the modern police force, the English have over and over again put their military into the impossible situation of acting as semi-policemen, standing between two communities, without identification of the enemy, as between Arab and Jews, Greeks and Turks, or Catholics and Protestants, each time with the same result. All Mr. Critchley's story illustrates the desirability for some graduated response in civil disturbances, but the English still do not seem to have learned the lesson that there is a period between the initial discomfiture of the police and the entrance into action of the military which still needs to be covered. One can only hope that Mr. Critchley's work will be read with close attention by his professional colleagues in the administration, so that at some future date they can turn their attention during civil disturbances to providing police support which does not require the immediate use of the military, and that they will approach their task with the same wisdom and tolerance as their predecessors did in the nineteenth century, and which Mr. Critchley still shows today.

Brian Chapman

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Freedom's Battle. By J. Alvarez del Vayo. (New York: Hill & Wang, 1971. Pp. xxxix, 371. $8.50, cloth; $2.95, paper.)

Apart from the addition of a new foreword by the author, which has little bearing on the rest of the volume, this is a reprint of the text, stylishly translated by Eileen E. Brooke, that appeared under the same title in 1940. Since then a second autobiographical work by Alvarez del Vayo has appeared in English: *The Last Optimist* (1950). Yet another work has been published in France under the bibliographically confusing title of *Les Batailles de la Liberté* (*Mémoires d'un Optimiste*) (1963). *Les Batailles* includes parts of both books in English and, together with this reprint, leads one to suppose that the author still stands by what he wrote in 1939.

Freedom's Battle covers the period from February, 1936 to autumn, 1939, during which the author was one of the most important Spanish Socialists. He was rather surprisingly made Foreign Minister in Largo Caballero's "Government of Victory" in September, 1936 and carried out his duties to the satisfaction of the Soviet Ambassador rather than the Spanish Republic's Premier, who nevertheless retained him until the fall of his ministry in May, 1937. From October, 1936 Alvarez del Vayo was also Commissar General of War until Prieto had him removed from the post in November, 1937 because of his philo-Communist activities. In April, 1938 he returned to the Foreign Ministry in Negrín's "Government of National Union" and again proved such a faithful supporter of Communist interests that he was later expelled from the Socialist Party. It would be difficult to describe his position since the Civil War as other than that of a "fellow-traveler."

What Alvarez del Vayo has to recount about the Spanish Civil War can for the most part be considered under two headings: the international aspect and internal developments within the Republican zone. With regard to the former, he contends that the Spanish Civil War was Germany's and Italy's rehearsal for the Second World War and he argues that there would have been no world war had the Western democracies been bold enough to stop the Axis in Spain. There is, however, no reason to suppose that a confrontation between the Axis and the democracies in or over Spain would have had less widespread repercussions than the confrontation over Danzig in 1939, while the explanation of the Spanish Civil War as the prologue to World War II is historically worthless. Alvarez del Vayo also argues that "no one should be able to deny that the collapse of the Spanish Republic was due to Non-Intervention" (p. 70), which allowed the Axis to bring about the victory of "a general who would be willing to become the instrument of Italo-German policy" (p. 115). Franco did not, of course, turn himself into an Axis puppet, and Alvarez del Vayo overestimates the extent of Axis aid, but his basic point about the importance and significance of the nonintervention policy for Republican defeat is correct. It would be going too far, however, to agree with him that there is no need to look for causes farther afield. The general bias of the author may be judged by the fact that, although he was writing after the conclusion of the Hitler-Stalin pact, he has not one word of criticism for the Soviet Union, whose "counter non-intervention" was, we are led to believe, quite altruistic.

Recounting a speech to British politicians in 1939, the author remarks: "I was careful to give more weight to the splendid struggle of the Spanish people than to our internal dissensions" (p. 321). A similar preoccupation shows through in these memoirs. Furthermore, what he has to say about the internal history of the Republican zone faithfully reflects his adherence to the Communist party line: Negrín (and Alvarez del Vayo) can do no wrong, whereas Largo Caballero, Prieto, "certain army leaders" and "small opposition groups" (e.g., the C.N.T.?) come in for varying degrees of criticism. The social revolution of July 1936, which the Communists were determined to liquidate, is scrupulously ignored here so as to make more attractive the thesis that the war was a "battle between democracy and Fascism" (p. 265). Readers are solemnly assured that "at no time did the Russian Government attempt . . . to interfere in internal Spanish politics" (p. 76); the author seems not to recall that Largo Caballero once ordered his Foreign Minister and Commissar for War out of his office for unpatriotic subservience to the Soviet cause.

After thirty years it is clear that *Freedom's Battle* was written to persuade rather than to inform. It is always good to have as many books in print as possible, and even propaganda works have their value, but one is left wondering why this particular work should have been reprinted.

R. A. H. Robinson

University of Birmingham

Southeast Asia Tomorrow, Problems and Prospects for U.S. Policy. By Melvin Gurtov. (Baltimore: The Johns Hopkins Press, 1970. Pp. 114. $6.95, cloth; $1.95, paper.)

The scope of this interesting, provocative, and modest size volume is even broader than its title suggests. The author not only deals with a *tour d'horizon* of the world of southeast Asia tomorrow but concerns himself with a variety of current and future political involvements of the major powers throughout the Pacific zone. Professor Gurtov understandably notes that his study "may be regarded as a point of departure for more detailed analysis," for the interrelated political concerns of some twenty-odd Asian states can hardly be surveyed within a scant 110 pages. He also suggests that his piece "offers one approach to assessing the implications of the prospective security environment over roughly the next decade in Southeast Asia for United States policies" (p. ix).

Given these ambitious outlines, the author is surprisingly successful in conveying a sense of the options, alternatives, and dilemmas facing the United States as it has begun to rethink its national interests and priorities in Asia. The book, however, is not a political science tract based on specific comparative materials, nor is it a polemic adapted to available empirical data, but rather a "think piece" dealing with concepts on a grand scale. Gurtov notes a wide range of Asian problems that involve choices and decisions for the United States. These choices, in the main, are enumerated, discussed, and simplistically described without much comment. The lay reader will find that he needs corroborative evidence for the intellectual arguments presented to him, while the specialized one will be conscious of a number of all-too-serious omissions in the discussions. These omissions, on subjects of importance bearing on American-Southeast Asian relationships require discourse, if only because these influence the attitudes of the People's Republic of China, Japan, and the USSR as these states position themselves in the changing Asian diplomatic and political spectrum. Little, if any, space is devoted to the complex present and future roles of the overseas Chinese in Asia and their cautious links with Communist China; hardly any note is taken of Japanese noncommercial initiatives in Southeast Asia; the convolutions of the USSR's diplomatic and military forward strategies in Asia are tangentially mentioned, and the recent Sino-American rapprochement is cautiously suggested as one of various alternatives in the author's broadly defined views of a future American political posture towards Asia.

Perhaps one should not ask too much of this policy-oriented survey, nor should the author be taken to task for his lack of prescience on individual issues and developments. And, despite the misgivings noted above, the study, initially published in May 1969 for the Rand Corporation as a Memorandum for the Department of Defense, seems so far to have stood up well to the passage of time. The book's contribution might usefully be considered in terms of an appreciation of possible diplomatic American maneuverability in the Pacific. Professor Gurtov describes well the broad elements of conduct available to Washington in pursuit of its elusive national objectives. As different parts of Asia go their own separate, sometimes regional and sometimes national ways, the United States will need to meet the challenges of the seventies by circumscribing and hedging its long-term objectives. In a clear and brief introduction to his list of American interests in Asia he mentions reference guidelines designed to bring the United States into fruitful and harmonious contact with Southeast Asia specifically and Asia in general. Thus he notes, *inter alia*, that America needs to:

> undertake no new bilateral or multilateral defense obligations, in the belief that the American commitment to the security of vital allies from hostile external domination is sufficient under existing treaties or pledges; . . .
>
> approach very cautiously any requests for assistance from new Asian military organizations which might be established; . . .
>
> reappraise priorities and funding alternatives for military and economic assistance, giving particular attention to multilateral institutions as alternatives to bilateral programs. (pp. 63–64)

Many of these trends are suggestive of policies the U.S. might consider once the various Vietnamese conflicts have been settled. On more specific grounds, the author occasionally leaves himself open to the charge that his information is distinctly dated or incorrect and his arguments are correspondingly diluted. Thus various comments on specific conditions in the late 1960s about individual Southeast Asian countries are now outdated. Changes in local regional or area political situations, as a result of governmental changes, civil disturbances, shifts in election results and the like, make sections of the book inapplicable. On various matters pertaining to future Asian diplomacy and development Gurtov is on shaky grounds. He maintains, for example, that "if Moscow substantially increases its economic stakes in Southeast Asia during the coming ten years, it would clearly be in American interests to have collaborative, though probably of necessity independent, assistant efforts in Asia play some part in the search for new avenues of detente" (p. 93). But this advice should be tempered with a cautionary proviso that individual members of the Asian subsystem are likely to appraise joint American-Soviet regional efforts with some apprehension and can be expected to act accordingly.

A final point: The author asserts in his concluding remarks that policy makers should try very hard to align U.S. values with particular foreign programs. His prescriptions are well taken, even if a bit obvious to the informed reader, as he notes that: "A vital American interst in protecting a nation against external aggression needs to be distinguished . . . from overidentification with particular regimes whose ineffectiveness or unsavory governmental processes run against the grain of American values" (p. 104). It is a pity that this volume could not have shed more light on possible Asian reactions to such sentiments.

RENÉ PERITZ

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The Mexican Economy: Twentieth-Century Structure and Growth. By Clark W. Reynolds. (New Haven and London: Yale University Press, 1970. Pp. 468. $13.50.)

Mexican Economic Development: The Roots of Rapid Growth. By Roger D. Hansen. (Washington: National Planning Association, 1971. Pp. 150. $3.50, paper.)

In this thoroughly researched, sympathetic, and cogently written volume, Reynolds has provided an outstanding account of Mexican economic growth. Building upon a comprehensive survey of Mexico's economic history from 1900 to 1965, he devotes major attention to structural changes in the economy, land reform and agricultural productivity, urbanization, trade policy, and public finance. One of the five appendices even presents a concise discussion of the opportunity cost of the Mexican Revolution. For those readers with little time to invest in the book, the epilogue will seem definitive in its succinct presentation of Reynolds's conclusions. These summary views are so well founded, and they raise such important issues of modernization, that all students of political development should consider them, in the course of doing which they may well decide to go back and examine Reynolds's earlier chapters as well.

The volume contains material of both narrow interest for Mexicanists and broader interest for students of development. In addition to new data on such frequently treated topics as Mexican land reform, Reynolds also points to less familiar findings, like the extraordinary productivity of Mexico's service sector. With broad relevance to other developing nations, he stresses that Mexico's commitment to the goals of the 1910 Revolution led to heavy government investment in agricultural and rural projects before the industrial push of the 1940s, so that, once industrialization gained prominence, it could draw on a balanced and self-sustaining setting in which agricultural expropriation and investment had already taken place. Although the greatest asset of Reynolds's study is the assembling and interpretation of fresh data, the author's sympathy for the Mexican *ambiente* and the vast literature concerning it also makes the volume stand out above others in the field.

To quibble over specific details of the book is not to detract from the essential soundness of its conclusions. Reynolds writes that Mexico "today accommodates over 40 million inhabitants," whereas, in December, 1970, when the book was published, the Mexican population had just reached about 50 million. While the extensive bibliography lists several studies published in 1969 and 1970, it oddly omits such important books as *Growth, Equality, and the Mexican Experience* by Morris Singer. The discussion of inadequacies in Mexican education (pp. 289–297) tends to exaggerate the failure of public investment, to neglect the impact of rapid population increase, and to pass over the manner in which the lag in higher education may have encouraged political passivity and stability. These points seem minor, however, when compared with the understanding that Reynolds demonstrates over and over again in analyzing the history of Mexican growth and its implications for the future of Mexico and the theory of development.

The Hansen book stands in contrast not only to the work of Reynolds and other social scientists but also to a longer study by Hansen himself. *Mexican Economic Development* is a shorter, less expensive, paperback version of Hansen's work entitled *The Politics of Mexican Development* (Baltimore: The Johns Hopkins Press, 1970). Both the Hansen books begin Chapter 1 with the statement that "Since the mid-1930s the Mexican economy has grown at an annual rate in excess of 6 percent," and, throughout subsequent chapters, they often remain identical in phrasing as well as in content. The data and footnotes of both books generally come from widely-read secondary sources, many of which are in English. Out of 164 initial footnote references to books and articles in the six chapters of *Mexican Economic Development*, for example, 126 references cite documents in English, 37 cite works in Spanish, and one cites a source in French.

Nevertheless, this small volume is an often insightful, if occasionally misleading and rather repetitive, analysis of Mexican growth. Hansen's greatest contributions are generally his specific criticisms of failures in other commentaries, such as his reminder that Mexico's *net* earnings on the two-way transfer of tourist funds between Mexico and the United States are far less than the gross receipts of gringo dollars spent in Mexico, which are usually cited as being so much to Mexico's advantage. On the other hand, Brazil's recent experience belies his assertion that the Brazilians have not been able to achieve a rate of growth equal to that in Mexico (p. 51). Hansen has given us a wealth of statistics, a series of intriguing, half-answered questions of causation, and sound personal judgments on such topics as the role of inflation and the influence of businessmen in politics. His cogent discussion of the regressive impact of Mexico's tax system is only partially outdated by the fiscal reforms of Luis Echeverría. Because the book is appealingly compact and low-priced, and because it employs a comparative framework, it should be useful in courses on economic development. In scholarship and depth of analysis, it is not the equal of the Reynolds volume, but it does not pretend to be.

Frederick C. Turner
University of Connecticut

British Tanganyika, An Essay and Documents on District Administration. By Robert Heussler. (Durham, North Carolina: Duke University Press, 1971. Pp. 154. $6.75.)

This short book—the essay consists of less than 25,000 words—asks certain basic questions about imperialism in Tanganyika (now Tanzania) from World War I until the early 1950s. The concern is with the imperialists, specifically with the D.O.'s (District Officers) who were responsible for administration. The author sets out to ask these questions (p. 7): "Who were the first political officers, what were their initial administrative tactics, what were the reactions of British and Africans to each other, what changes took place?"

Succeeding chapters give partial answers to these questions, dealing with the variety of interests of D.O.'s, the problem of a small staff, and the rise and fall of Governor Sir Donald Cameron's policy of Indirect Rule. The last chapter deals briefly with the resentment felt by some D.O.'s against "detribalized" and educated Africans.

The documents consist of tour reports and miscellaneous notes by contemporary D.O.'s. Intended to give "a vivid, precise and wide-ranging picture of the countryside and its people," the papers suffer from having inadequate explanatory notes.

The author describes the book as "a minor contribution" (p. 6), so it might appear unjust to criticize him for not having written a different book. Yet Robert Heussler *is* an authority in his chosen field, British colonial administrators; he *is* the author of two first-rate books, *Yesterday's Rulers: The Making of the British Colonial Service* (Syracuse University Press, 1963) and *The British in Northern Nigeria* (Oxford University Press, 1968). His second book deals, in much more satisfactory detail than does the one under review, with such topics as relations between ruler and ruled. For in his latest book, the author offers no comparative framework, no analysis, no theoretical considerations. Given this lack, he appears to endorse the rather stereotypic views of his subjects—"a poor type of Sikh" (p. 32); "the Masai is a gentleman" (p. 34); "native psychology" (p. 84). (In parenthesis, I add that as a Tanganyikan D.O. of the 1950s, I found a wider range of personalities among my colleagues than is suggested in this book).

Also, given the increasing difficulty in getting permission to consult the National Archives of Tanzania, it is disappointing to find such a slight result from one who did have access. While the book will be of some interest to students of colonial administration, it is not in the same category as other recent studies such as J. Gus Liebenow's *Colonial Rule and Political Development in Tanzania: The Case of the Makonde* (Northwestern University Press, 1971, especially Chapters 4–7), which presents an analysis of a specific area of Tanzania; nor is it in the class of one of the best overall accounts of British colonial administrators, Michael Crowder's chapter "The White Chiefs of Tropical Africa," in *Colonialism in Africa,* Vol. 2 (ed. L. H. Gann and Peter Duignan, Cambridge University Press, 1970).

British Tanganyika is more like J. C. Cairns's *Bush and Boma* (John Murray, 1959, the life of a Tanganyikan D.O.) or like such illuminating novels as Joyce Cary's *Mr. Johnson* or Chinua Achebe's *Arrow of God,* in supplying useful background rather than any profound explanations.

David Brokensha

University of California, Santa Barbara

The Order of the Death's Head: The Story of Hitler's SS. By Heinz Höhne. (New York: Coward-McCann, 1970. Pp. xii, 690. $12.50.)

The Rescue of Danish Jewry: Test of a Democracy. By Leni Yahil, translated by Morris Gradel. (Philadelphia: The Jewish Publication Society of America, 1969. Pp. xx, 536. $6.50.)

Heinz Höhne's "fully documented account of the most macabre elite in modern history" invites scholarly scrutiny, for the author, a senior journalist with the German newsmagazine *Der Spiegel* (where it first appeared in serial form) has castigated earlier authors on the difficult subject of the SS.

Mr. Höhne's grand picture of the struggle for power among overlapping and competing groups within the Nazi realm is colorful and dashing, but also, at times, shallow and injudicious. Let us consider several cracks in the seemingly solid structure of the book.

The author writes that "ever since the SS had become the Führer dictatorship's primary instrument, its leaders had supported a Jewish policy differing in many respects from the Party's crude anti-Semitism"; namely no physical liquidation was envisaged, but rather a hastening of the expulsion of the Jews, which also meant encouragement of the Zionist work fostering Jewish emigration to Palestine. To call this a policy peculiar to the SS (pp. 325 ff.) is incorrect. As early as September 1933 the German Economics Ministry had reached a clearing agreement with the Jewish Agency encouraging this emigration, and even before 1933, certain racialists, such as Hans F. K. Günther, had praised the Zionists, thus demanding a clear separation between Germans and Jews.

Mr. Höhne sympathizes with the cynical, "level-headed" intellectuals, who flooded into the SD, the great ideological information agency. Thus he dwells on Otto Ohlendorf, the SD Chief and notes only that "he directed the murder of 70,000 Jews" (p. 423). The source had correctly given the number as 92,000.

In another minimizing distortion Höhne calls the murder of 600 Jews in Galicia by the SS Division "Viking" a reprisal for Soviet crimes (p. 469), but his source had put the SS "reprisal" excuse in quotation marks, because, clearly, entirely different people were involved. Mr. Höhne gives no hint of the wartime collaboration between Haj Amin el Husseini and the SS, when the Grand Mufti helped to activate the 13th SS Division, composed of Bosnian Moslems.

The author insists that for the SS Himmler had used the Jesuits as the model (p. 144), yet fails to mention the primary model: the German Knights Order (for details, see the reviewer's "Constraint and Choice in the SS Leadership," *West. Pol. Quart.* Vol. XVIII [1965], pp. 551 ff.).

The coverage of the SS ideological bureaucracies, for example, the SS Research and Teaching Institute Ahnenerbe and Lebensborn, the "Fount of Life" Society, (both of which Himmler's circular of January 1942 had classified as "war important institutions") is sketchy. Ahnenerbe is mentioned as primarily concerned with old Germanic relics but its role in replacing pre-Hitlerian teaching and scholarship with underpinnings of Nazi ideology and its infamous "military scientific research" (for which the former director was tried and convicted at Nuremberg) is overlooked. The rate of infant mortality in the Lebensborn maternity homes for unmarried mothers is specified, but not Lebensborn's kidnapping of "racially valuable children" of non-German nationalities, sterilizing members of "inferior races," etc. The author's facile view—that SS ideology impressed only Himmler and the Nuremberg prosecutor—offers an explanation, but no excuse. Mr. Höhne does not miss assuasive points; for example, he notes that the SS leaders of the Jewish massacres hardly came voluntarily and that most wanted to get out of the East. And the grim chapter on the genocide program, with which the SS is inseparably linked, ends with a brief summary of the dramatic story of the rescue of the great majority of the Danish Jews in October 1943, because for Mr. Höhne the part of Werner Best in sabotaging the deportation plans is but "the most striking example of the scepticism from which many SS leaders were now suffering" (p. 399).

This rescue is vastly more important for Dr. Leni Yahil, a modern historian teaching at the Hebrew University, and even Best emerges in a different light. Her book, the most scholarly study available in English, is well balanced; she has tried not to overdo either her enthusiasm for the Danish rescuers nor her hatred for the German persecutor.

Her chronicle begins with the history of the Jews in Denmark from the Seventeenth Century to modern times, especially the 1930s. Particular emphasis is placed on the years of the German occupation and on the launching of anti-Jewish measures; the central portion of the book deals with the rescue operation and its aftermath.

Dr. Yahil shows how Werner Best, the German plenipotentiary, and the other German occupation forces were engaged in a struggle for power, while the Danes tried to preserve as much freedom and independence as possible. She stresses that Best appreciated that his own success as an administrator depended on his ability not to incite the Danes, who had opposed the policy of deporting "their" Jews. But the author also shows that the German armed forces (including the Navy) were opposed to the deportation, and she places the responsibility upon Best, who through the measure wanted to increase the German police forces, have them under his command, and also to have the Army place supporting field intelligence forces under Best's orders. Thus Werner Best is demonstrated to have pursued a two-faced policy, dictated on the one hand by his understanding as an administrator and on the other by his ambition as a high SS leader.

Dr. Yahil, thoroughly in command of her varied sources, carefully separates truth and fabrication in Best's various statements—a happy contrast to Mr. Höhne, who strongly relies on Best in his summary of the Danish events. Dr. Yahil's appended "factual, ideological and psychological analysis" of Best's pre-Danish period is noteworthy and still timely (since November 1971 Best is again facing trial in Berlin for his part in the Jewish massacres in Poland); this probe is on the whole, excellent. She does not realize, however, that when Best advocates genocide and speaks of "the laws of life," this is not his own "somewhat foggy notion" (p. 414) but rather a key Nazi ideological term, which appeared *ad nauseam* in SS guidance pamphlets.

For Dr. Yahil the rescue is primarily a political, not a humanitarian, problem. As the subtitle indicates, the small democracy of Denmark defended both its Jews and its fundamental political and social structure against the overwhelming might of a totalitarian giant. The idea that persecution conflicts with "the judicial conscience" had been transmitted through centuries of Danish culture, and through the teachings of the nineteenth-century Danish priest Nikolai Grundtvig and the Folk High School system; ideas such as this constituted a bulwark against Nazi thoughts and attitudes.

In the successful rescue of 7220 Jews from

Denmark, nearly all sections of the Danish people stood ready to help; in the course of the rescue operation, a network of contacts and channels was developed which aided the general anti-Nazi underground. The author attributes this success not to the special occupation context, nor to Denmark's proximity to neutral Sweden, nor to the fact that persecutions began when the tide of war had already turned against Germany, but instead primarily to the special character and moral stature of the Danish people and their love of democracy and freedom with responsibility.

MANFRED WOLFSON

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The Growth Potential of the Japanese Economy. By Kenneth K. Kurihara. (Baltimore and London: The Johns Hopkins Press, 1971. Pp. 148. $7.50.)

Professor Kurihara is known principally as a distinguished economic theorist specializing in Keynesian economics. Although he left Japan as a precollege youth before the Second World War, his interest in the Japanese economy as a testing-ground for some of his macroeconomic hypotheses has been evidenced in several journal articles. One of these is reprinted as an appendix to the present volume and attests to the high quality of analysis and insight that characterizes Kurihara's approach to Japanese economic problems. Kurihara revisited Japan as a Fulbright scholar in 1965, and many of the chapters evidently were written shortly after that visit, as may be seen from the data which largely reflect the developments in Japan during the 1950s and the early 1960s.

The book addresses itself to "the fundamental forces governing the speed and pattern of Japanese economic growth not only at present but in the decades ahead" (p. ix). In simpler language of economic analysis, this boils down to two questions: (1) Why has the Japanese growth rate been so high as often to be dubbed "miraculous" during the last 25 years? (2) Is this extraordinary growth rate likely to continue during the rest of the twentieth century?

Kurihara's Keynesian economics gives a quick answer to the first question by pointing to the overriding importance of the strong demand for investment in the private sector of the Japanese economy. This answer leads to a related question about the availability of resources for investment. Were there sufficient savings to begin with in the wartorn economy of postwar Japan? No. Therefore, a unique and interesting feature of the Japanese economy emerged. Investment has been financed primarily by aggressive credit creation by banks. This would have resulted in an acute price inflation in other countries, but in Japan prices have been kept stable largely because Japanese consumers have been willing to save increasing portions of their rising incomes. In other words, there has been a tacit commitment by all Japanese to a policy that urges "spend now and save later" in so far as "spending" is for capital formation. The amazing thing about it all is that this fine-tuned resource allocation has worked smoothly without planning or coercion but as a simple byproduct of the natural and informal consensus in the Japanese society. There have been many so-called "economic plans," but their efficacy has been practically zero. This picture of Japan's economic growth process emerges from Chapter 4 (Shifting Patterns of Consumption and Saving), Chapter 5 (Private Investment and Social Overhead Capital), Chapter 7 (The Fiscal-Monetary Milieu for Rapid Growth), and Chapter 9 (Economic Growth Without Secular Inflation). Of these, Chapter 7 is most important.

With respect to the future of Japan's economic growth, it is difficult to obtain a consistent view of it from this book. This is somewhat disappointing for a book on "growth potential." At least one answer to the question of the future of Japan's economic growth may be derived from the answer to the first question. One sure result that anyone would expect from 25 years of extraordinary capital formation in the profit-pursuing private sector is excess capacity. Kurihara points out that there are signs of excess capacity and that the private sector's demand for investment may slacken in the future resulting in slower economic growth (pp. 41–44). The worsening of labor shortage, he says, may also be a drag on growth performance (Chapter 6). But there are countervailing forces, too; for example, a possible rise in the propensity to consume may stimulate the demand for output (pp. 26–27), an increase in the efficiency of capital through technological progress may occur so that smaller investment would still be associated with larger output (Chapter 8), a further increase in labor productivity through the improvement of the social overhead capital would have a bearing upon labor efficiency (Chapter 5), and international competition may increase so that growth performance need no longer be held down because of occasional balance-of-payments crises (Chapter 10). How the balance of forces and counterforces comes out is not clear.

Kurihara's affection for the country of his origin vividly emerges from the pages of this book. One regrets only the absence of any critical evaluaion of the human and social consequences of economic growth. So long as certain economic events are explicable in terms of macroeconomic analysis, Kurihara appears to justify those events as good. Because it has been extraordinary and even miraculous, Japan's economic growth has created extraordinary social costs at home and enormous international complications abroad. It is hoped that Kurihara will soon realize the importance of these issues and apply his powerful tools of economic analysis to their elucidation.

KOJI TAIRA

University of Illinois

Spear and Scepter: Army, Police, and Politics in Tropical Africa. By Ernst W. Lefever. (Washington, D.C.: Brookings Institution, 1970. Pp. 251. $6.50.)

In 1962 a prominent political scientist and specialist on Africa wrote that "the question of civilian supremacy has not yet become a serious issue in the new states of Sub-Saharan Africa." Some ten years and numerous coups, plots, and mutinies later the military has come to be viewed as one of Africa's most politically significant institutions. Unfortunately, the tardiness with which Africanists came to this awareness has resulted in a rather meager output of scholarship concerning this extremely important aspect of African politics. The book under review is among the first published in America to deal specifically with the topic, and is clearly intended by its author and publisher to help fill the research void.

As such, at first glance the *Spear and Scepter* by Mr. Ernst Lefever appears to hold out great promise. Unfortunately this promise remains largely unfulfilled. The volume has as its main purpose, according to the author, the analysis and assessment of the impact of the armed forces on politics in tropical Africa. It focuses on three countries—Ghana, Congo-Kinshasa, and Ethiopia—and makes an effort to generate and defend a number of general analytic propositions by comparing them. Material on the three countries is presented in separate case studies that are largely descriptive, and lacking in any explicit common focus or integrating themes. The descriptions are superficial and based almost entirely on widely available secondary sources. At times the appropriateness of these sources is open to question, as when a book written by a participant in the first Ghana coup is heavily relied on for a description and analysis of civil-military relations during the Nkrumah regime. The consequence, in this case, is a chapter on the role of the military in Ghanaian politics that is sharply slanted toward the point of view of the officers who overthrew Kwame Nkrumah.

If one is expecting from *Spear and Scepter* some fresh insight into the role of the military in the politics of new states—or some new evidence on some old insights—he will be disappointed. In the only chapter of the book that attempts to be explicitly analytic, Lefever presents, and tries to demonstrate the validity of, twelve hypotheses regarding the military in politics. We are told, for example, that the armies of the three states in the study contributed to political and economic development because they are symbols of state in which the populace has great pride; because they are more "detribalized" than the general population; and because they have developed skilled manpower on which the civilian polity and economy can draw. Propositions of this type have been prominent in the literature on political development and modernization for the past decade. We would have hoped, therefore, that in a work of this kind they would not merely be restated, but that evidence would be brought to bear to permit assessment of their validity. But instead we are left with little more than assertions. *Spear and Scepter* contains very little of the type of evidence that would permit inferences, let alone conclusions, about the orientations of national populations toward their armed forces, about the relative absence of ethnicity within the military, or about the degree to which skills developed in the military have actually been utilized by civilian polity and economy.

Of those propositions that can be said to originate with Lefever, the most striking characteristic is their weak explanatory power. As examples, let me quote two of them, and the reader can make his own judgment:

> As long as the central government is effective and legitimate . . . the armed services make their most effective contribution to political stability and development by upholding the regime (p. 197).

> When a regime is too weak, corrupt, or arbitrary to govern, and there is no legitimate political agency capable of taking over, effective military intervention, . . . depending on the performance of the interim regime, can facilitate constructive political development (p. 197).

Although the author does not utilize an overall analytic framework, one theme does pervade the entire work, and that is Mr. Lefever's apparent distaste for the communist world and admiration for the West. This frame of reference intrudes at many places. It is the basis for an effort to classify African states according to a crude distinction between militancy and moderation, based not upon an analysis of the decision-making process or of policies actually followed, but rather upon the orientation of a given state toward the West and its political and economic models. "Moderate policies," writes Lefever, mean "continued cooperation with Western states and the adaptation of Western governmental forms to local requirements" (p. 29). And, of course, "moderation" is in all situations to be preferred to "militancy." Using this frame of reference, Lefever makes numerous statements, unsupported by any argument or evidence, that from outside the framework appear startling and at times incomprehensible. Thus, for example, we are informed that in the Congo and Ghana, Moscow made an effort to "radicalize" politics "by the classic communist tactics of subversion, through trade, loans, military aid, and bribery" (p. 14). How these classic communist tactics of subversion differ from the classic Western methods of influence we are unfortunately not told. Elsewhere we are informed, without the benefit of supporting argument, that in the Congo the Stanleyville regime was "the most serious danger to the integrity of the Congo in its first years," Katanga by comparison being a "far lesser danger" (p. 102).

There is one last troublesome aspect of *Spear and Scepter* that cannot go unmentioned—the author's tendency to make denigrating statements about African culture and history for which he offers neither empirical evidence nor logical argument. Although a number of examples deserve extended comment, space permits a discussion of only one. In a brief analysis of the African role in the history of colonial imposition Lefever includes the following passage:

> Physically and politically, tropical Africa was passive: . . . A female continent, Black Africa was to be discovered, penetrated and dominated by others. There were few exceptions to this image of passivity (p. 3).

Now one may quibble with the analytic usefulness of the "female" metaphor, or one may take offense at what may appear to be the intrusion of "male chauvinism," but what is really astounding is the author's failure to take account of the substantial historical scholarship on the subject—scholarship that has clearly shown the responses of African peoples to colonial imposition to have been extremely varied and in many cases the very reverse of passive. Numerous African peoples fought fiercely to maintain their independence, some engaging in guerrilla operations for as long as a decade. Moreover, research has shown that even those peoples that did not forcibly resist the intrusion of colonial control often acted out of a calculation of their political and economic interest within the African international system of the period—a calculation which, without the benefits of hindsight, was often highly rational.

I do not fault Mr. Lefever because his observations on African culture and history may be perceived as offensive by Africans, or because some of them contradict the overwhelming consensus of research carried on during the past two decades, but rather because he fails to acknowledge and come to terms with this information. When denigrating statements about a people's culture and history are presented as if they had face validity, they take on the appearance of ethnocentrism and chauvinism, rather than scholarship.

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Rebellion and Authority: An Analytic Essay on Insurgent Conflicts. By Nathan Leites and Charles Wolf, Jr. (Chicago: Markham Publishing Co., 1970. Pp. 174. $5.95.)

Rebellion and Authority has all the trappings of contemporary social science. A political scientist (Leites) and an economist (Wolf) team up in an interdisciplinary effort to analyze a major area of contemporary interest, insurgency and counterinsurgency (labeled rebellion and authority, respectively, in an unconvincing effort to neutralize the terms). Claiming balance and objectivity in treating the two sides, the book is laced with a profusion of social science jargon, especially from economics. It is studded with hypotheses, propositions, and counter-propositions.

The substantive burden of the work is well summarized in a blurb on the dust jacket:

> The broad aim of this book is to develop a method of analyzing insurgent conflicts—conflicts between Authorities and Rebellions—which approaches the rigor achieved in the analysis of strategic nuclear conflicts. Various concepts are developed, such as the "demand" for and "supply" of rebellion; the "inputs" and "outputs" associated with rebellion; "damage-limiting" and "profit-maximizing" influences on population behavior; and the principles of target selection as they affect the contesting sides. The concepts are applied to specific cases of insurgent conflict on foreign and domestic battlegrounds.

The principal conceptual innovation suggested in the work is a shift of focus from "demand" (popular support) to "supply" (resources): the book stresses the centrality of efficiency and organization for both rebellion and authority rather than popular preferences for the two sides. To this end, the authors examine and reject the "hearts and minds" view of insurgency and set forth an alternative framework that considers insurgency as "a system and an organizational technique."

Insurgency-as-a-system has three principal components: (1) the availability of inputs—personnel, supply, intelligence—and the costs of obtaining them from internal or external sources; (2) the means for converting the inputs—through indoctrination and training, for example—into activities or outputs; and (3) the targeting of the outputs against the existing regime.

Counterinsurgency-as-a-system has three corresponding concerns: (1) impeding the availability of the insurgent inputs and the efficiency of the conversion process; (2) destroying the insurgent outputs already produced; and (3) strengthening the organization and efficiency of the governmental authority so as to withstand the rebellion's onslaught.

The principal conclusion reached is that

> for R[ebellion] to win, it *need not* initially have the spontaneous support, sympathy, or loyalty of the people, not even of a significant minority of the people, although it may in fact enjoy such support. Moreover, even fairly far along in the struggle, R can make substantial progress *without* substantial popular endorsement. . . . Thorough organization and effective coercion can enjoin or engender particular modes of behavior by the population . . . (p. 149, emphasis in original).

In other words, effective organization can create public support—or indeed serve as a substitute for public support.

The main contribution of *Rebellion and Authority* is the skillful application of economic theory to the analysis of insurgency. This application, however, rather than adding to our knowledge of the subject or shedding new light upon it, simply recasts existing knowledge in different terms. The work is a prime case in which the application of economics jargon to the study of insurgency succeeds in jargonizing the subject more than elucidating it. As such, *Rebellion and Authority* is only peripherally successful as an interdisciplinary venture. By the same token, it fails in its articulated objective of raising the level of analysis of insurgent conflict to that of strategic nuclear conflict.

One might note in passing that, although the analysis of nuclear conflict enjoys a certain game-theoretical rigor, it fortunately lacks empirical content. By contrast, the analysis of insurgent conflict can rely on considerable empirical evidence, but its theoretical rigor has not progressed beyond the writings of Mao Tse-tung, Vo Nguyen Giap, Che Guevara, and Regis Debray. The analysis of insurgent conflict has yet to find its Herman Kahn or Thomas Schelling.

The central conclusion of *Rebellion and Authority* is not supported by historical or empirical evidence. While certain aspects of certain insurgencies at certain times may fit certain aspects of the Leites-Wolf analysis, there is not a single case on record that fits the overall framework. In particular, to my knowledge no insurgency movement has ever succeeded in the absence of popular support, its organizational effectiveness notwithstanding. (It should perhaps be noted that Leites and Wolf are themselves divided on the issue: Wolf stresses supply while Leites assigns equal weight to supply and demand. However, since the framework rests primarily on supply, one is surprised to read in the Preface that "both authors fully endorse the analytical approach of the book as a whole.")

Finally, the work is not as objective and balanced in its treatment of the two sides as Leites and Wolf claim. Indeed, the authors themselves are moved to write in the Preface: "Sometimes the posture of the [governmental] authority and, specifically, of U.S. policy in relation to authority, is adopted more completely than perfect balance would warrant" (p. vii). It may be relevant to note that the study was supported by The RAND Corporation, which also holds the copyright to the book.

MOSTAFA REJAI

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Communications and National Integration in Communist China. By Alan P. L. Liu. (Berkeley: University of California Press, 1971. Pp. 225. $8.50.)

I accepted the task of reviewing this book with great expectation. This was partly because of my personal interest in the subject and partly because of the publisher's promotional literature, according to which this work not only provides a "comprehensive" account of the development of the mass media in China since 1949 but also is "the first study to tie together considerations of (1) comparative politics, (2) the relationships between the mass media and the social system, and (3) the concept of totalitarianism."

Upon seeing the book, however, I was swiftly disillusioned. Except for a few quotations of marginal relevance from such prominent social scientists as Karl W. Deutsch, Alex

Inkeles, and Edward Shils, nowhere does the author bring the above-mentioned "considerations" or "concepts" to bear on his work at hand. Nor does he seriously attempt to explore the complex policies, attitudes, and behavioral patterns that Peking has been promoting through the mass media with the objective of achieving national integration based on socialism. Instead, he devotes the bulk of the 181 pages of the text to a cursory discusion of the "organization and conduct" of the mass media (radio broadcasting, newspapers, books, and films) and their work during the mass campaigns; and his treatment of the roles of the mass media in national integration is even more casual.

What is more, his book is replete with factual errors, contradictory accounts of events, inconsistent evaluations, questionable interpretations, and a virulent anti-Mao bias. One of the glaring factual errors is the assertion (p. 138) that the content of newspapers "from 1963 to the breakout [outbreak?] of the Cultural Revolution in 1966 showed no dominant themes," whereas in reality the press in that period was deeply involved not only in the campaign to emulate the army and in the intensifying polemic against Soviet revisionism but also in the "three revolutionary movements" (the class struggle, the struggle for production, and scientific experimentation), the "recall bitterness" campaign, the polemic against the philosophies of Yang Hsien-chen and others, and the concerted efforts to "foster successors to the cause of the revolution" and to extol the virtues of self-reliance and serving the people. Typifying the multitude of contradictory accounts of events are two sentences on p. 99: one of them states that it was after 1963 that the army assumed the responsibility of building up Mao's prestige, whereas the other dates the beginning of that effort back to 1960. A good example of Liu's inconsistency in evaluation is his unqualified castigation of Mao for his reliance on mass campaign as a means of conducting propaganda (pp. 53 & 110–111) and his praising of the then "revisionist"-controlled Propaganda Department for having employed the same instrument in inculcating new values in the minds of a vast number of Chinese people (p. 86). Among Liu's many questionable, if not clearly erroneous, interpretations is his assertion (p. 33) that what led Mao to "destroy" the printed media during the early stage of the Cultural Revolution was those media's ineffectiveness as an instrument of mass mobilization which Mao, according to the author, had recognized since the Great Leap Forward. Regardless of how Mao really assessed the printed media's role in the late 1950s, it has been the consensus of most "China watchers" that the Maoists closed down large segments of the printed media in 1966–67 largely because they had been under the control of persons "taking the capitalist road." Liu's passionate anti-Mao bias is evidenced by his charge that Mao has never been willing to "improve the material conditions of peasant's [sic] life" (p. 173). Anyone with some knowledge of Mao's rural policies both before and after the Cultural Revolution is likely not to believe his eyes while reading such statements.

At least two major causes of the book's errors of commission and omission are easily detectable. First, despite the author's assertion that he "has depended mainly on original Chinese Communist publications, particularly newspapers and journals," and that he believes he has "exhausted all sources that are publicly available in the United States and in major research institutes in Hong Kong and Taiwan" (p. xvii), he has actually relied to an unusually large extent on secondary sources, especially anti-Communist publications. For example, his numerous references to the Cultural Revolution seem to be based almost exclusively on Chao Tsung's three articles on the subject in *Tsu Kuo* (an anti-Communist magazine in Hong Kong) which he cited no fewer than 22 times in a single chapter. Nor was his use of accessible primary sources indeed exhaustive: most of the Chinese Communist newspapers and journals which have been used by earlier researchers in the field are not even listed in his bibliography. Insufficient care on the part of the author and his editors is seen in incorrect transliterations of some familiar words (e.g., Ta Sai Commune for Ta Chai Commune), inconsistent footnote style and lack of uniformity in translating a number of proper names (e.g., The All-China Federation of Trade Unions is variously translated as All-China Labor Union and All-China Labor Union Federation.) These, of course, are matters of relatively minor consequence in comparison with those mentioned earlier.

In conclusion, this book contains very little merit but much misinformation.

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Parliaments: Change or Decline? Edited by Gerhard Loewenberg. (Chicago, New York: Aldine-Atherton, 1971. Pp. 200. $7.95, cloth; $2.95, paper.)

The challenging title of this collection of essays suggests that they confront systematically a topic seldom explored in depth in other works on legislative assemblies. At a minimum, the title leads one to expect an attempt to interrelate perspectives on the changing role of parliaments in political systems. Instead, the reader will find a *mélange* of approaches to the topic of the functions, prescribed and actual, of some contemporary parliaments. As such, Loewenberg's book might be useful reading in comparative politics courses. As a contribution even to the meager scholarly literature on the theme suggested by the title, however, the volume falls short of the mark.

Regarding the usefulness of the book as a text, all except one of the essays (Wahlke's article, excerpted from the *British Journal of Political Science*) are not readily available in their original form. John MacIntosh's chapter, arguing for the institutionalization of specialized committees in the British House of Commons, appeared previously as an occasional paper. Wilhelm Hennis's piece on the Bundestag, in which the author attributes to committees the usurpation of the debating and opinion-structuring functions of parliament, appeared originally in German. François Goguel's and Philip Williams's essays on France, presenting divergent views on the "reforms" of 1958, are both adapted from papers delivered at the Princeton Conference on the Future of Legislative Power. Henry C. Hart's diachronic study comparing the role of parliament in nationbuilding in India and England (1825–1850) is a condensation of a paper delivered at the 1969 *APSA* meeting.

A well-chosen excerpt from Bryce's classic immediately follows Loewenberg's introduction. The selection contains enough of Bryce's observations on late nineteenth- and early twentieth-century legislatures to illustrate that the suspicion that something is wrong with parliament is not a novel one.

Wahlke's article, seemingly intended to serve as a concluding chapter in lieu of a conspectus by the editor on the viewpoints presented in the volume, deserves special comment. After succinctly reviewing much of the literature on the ascribed functions of legislatures, Wahlke argues for a redirection of conceptualizations of legislatures. Specifically, he suggests that instead of assessing legislatures' performances in their roles as conversion mechanisms in simple demand-input models, we may benefit from a focus on the roles of legislatures in mobilizing and maintaining diffuse support for the political system. The strength of Wahlke's argument is in pointing out the "declining legislature," for the straw man it often is. The problem with utilizing Wahlke's essay as a concluding chapter, however, is that none of the other essays seems to rely explicitly on a demand-input model for its force. Rather, the importance of the essays in terms of the decline of parliament may be seen more clearly from K. C. Wheare's eclectic perspective on the problem. As Wheare noted in the concluding chapter of his well-known work on legislatures, much of the underbrush surrounding discussions of the alleged decline of parliament could be cleared by an explication of the standards used to assess the importance of parliament. In what *way* has parliament declined; power, efficiency, style, public esteem, or public interest? Also, Wheare reminded us, we must indicate the temporal and spatial and institutional context of the declining significance of parliament.

The diversity of the perspectives of the essays in Lowenberg's book obviates the necessity of raising the questions Wheare suggests. When Bryce wrote of the decline of parliament, he had in mind such related things as the style of debate, the personnel of parliament, and the public's esteem for M.P.'s. He lamented the changes that had taken place with respect to these attributes from the "days of Melbourne and Peel."

Goguel and Williams compare the Fourth and Fifth Republics in France in terms of changing legislative-executive relationships. Williams clearly sees the changes as representing a decline, assuming, as he seems to, that the proper functions of parliament are to persuade a government to change its mind or to mobilize the electorate or both. Goguel notes the diminution of legislative power, mainly brought about by recent crises and the personality of De Gaulle, but he does not evaluate this development.

John MacIntosh and Wilhelm Hennis disagree on several points, chiefly on the efficiency of specialized committees in facilitating what each author considers the correct functions of parliament. MacIntosh's concern is the increasing inability of parliament since the nineteenth century to control the executive effectively. He sees committees as a counterweight to increases in executive autonomy, and, relatedly, he argues that debate was improved during recent limited experiments with specialized committees in the British House of Commons. Hennis, on the other hand, persuasively argues that a stress on the role of committees is misplaced owing to a misconception that parliament should be "productive" and to historical illusions about "parliamentary supremacy." He contends that committees have arisen as structural equivalents to bureaucratic organizations: they do not do anything that could not be done as well by other institutions. What they cannot do and what they obstruct, is the influencing of popular opinion through parliamentary debate.

The validity of Hennis's argument and of MacIntosh's argument clearly rests on assumptions about the attentiveness of the public to the doings of parliament. Obviously, at some level, the arguments of Williams and Bryce also assume a certain level of public attentiveness. The data adduced by Wahlke, however, raise serious doubts about the empirical validity of these assumptions.

The reviewer's task is not to integrate the positions of the authors nor to apply a framework to them. Unfortunately, neither the interstitial material nor Loewenberg's opening essay bridges the gaps among the diverse perspectives of the essays—essays that are informed by varying analytic and polemic orientations and that refer to different national experiences. By way of illustration, Loewenberg does not try to justify the inclusion of Hart's article on India other than to observe that it is interesting to look at cross-temporal differences in expectations of parliaments.

It is true that the introductory essay is "wide ranging," as the book's jacket promises. Indeed it is a literate synthesis and good categorization of the "crises" that parliaments have faced. Yet, the introduction is just that, a cursory attempt to mention all the essays in light of their bearing on crises in modern parliaments but with no serious attempt to articulate them to the overt theme.

The principal utility of Loewenberg's book, in my view, is as a text or supplementary reading for comparative politics courses. But, for purposes of offering something new in the way of a statement on the problems of the changing role of legislatures in spatial and temporal context, the essays are inadequate. And, in light of the superior quality of Loewenberg's own work on parliament, the introductory chapter is especially disappointing.

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The Structure of Canadian Government. By J. R. Mallory. (New York: St. Martin's Press, 1971. Pp. xii, 418. $11.50.)

The "quiet revolution" in Quebec in the 1960s cast doubt on the traditional view of Canada as one of the "Anglo-American democracies" but provided no alternative classification that recognized the changing status and aspirations of French Canadians. If Canada can meet Quebec's demands and create a new form of continental polity, its multicultural and bilingual evolution based on a parliamentary confederation of ten unequal provinces may prove a model for the new multicultural European Community of ten unequal parliamentary states.

The approach of many scholars to Canadian government has been somewhat conservative, in the tradition set by R. MacGregor Dawson's *The Government of Canada* (1947), the fifth edition of which, edited by Norman Ward, appeared in 1970. Professor Mallory does not attempt to break new ground comparatively or conceptually: like Dawson he takes as his starting point the Anglo-Canadian tradition of Monarch, Governor-General, cabinet government and parliamentary sovereignty suitably modified by the federal structure adopted by the framers of the British North America Act in 1867. In Chapter 5 he deals elegantly and economically with certain aspects of political behavior ("The Electorate"), but this is a minor feature of a study that for the most part prefers politicians and mandarins to voters and lobbyists. Yet there is an interesting contrast between the historical treatment of "The Pattern of the Constitution" in Chapter 1 and the contemporary analysis of the last chapter "Stability and Change: Mechanism of Adjustment in a Federal System." Throughout the book, French Canadian scholars receive as little attention as Americans.

In examining some of the more complex issues in Canadian politics Mallory exhibits the sure touch of one who has studied them closely over many years. He has done for Canada what the judicious Jennings did for Britain, though on a smaller canvas. For instance, in his dispassionate handling of the 1926 controversy between the Governor-General, Lord Byng, and the Prime Minister, Mackenzie King, he avoids the partisan polemics that have almost obscured this important event, recognizing the faults on both sides.

Some important contemporary issues are ignored or glossed over. Among those ignored is the debate about control over Canada's economy; among those glossed over is the failure of the upper house adequately to represent the provinces and regions of Canada—an unfortunate situation in the light of the country's present French Canadian problem. The reader is somewhat compensated, however, by the long discussion in Chapter 4 of Canada's federal administration, a field in which the author is preeminent. It is shown to have many unusual features, among them the key role of senior civil servants in Ottawa (called "deputy ministers" in Canada when they reach the top)—a role even more crucial than at Westminister. Furthermore, Canadian Crown Corporations from Canadian National Railways to the Canadian Broadcasting Corporation and Air Canada distinguish Canada from its American neighbor, though their creation owed little to socialist beliefs and much to the need to preserve the country's identity. Among other distinctive features of Canadian administration is a Treasury Board responsible for the management of the public service and presided over by a Minister. There has been an attempt to make the service more businesslike following the report of the Royal Commission on Government Organization, and more bilingual following the reports of the Royal Commission on Bilingualism and Biculturalism: Mallory's analysis of their implementation could well be extended.

As a textbook, this volume is a little old-fashioned. Certain offices and institutions, for example, the Comptroller of the Treasury and the Speaker of the House of Commons, are discussed in terms no longer appropriate. The committee system of the House of Commons is explained as it used to operate, the recent changes then being added. The Exchequer Court is described in the present tense, although subsequent paragraphs indicate that it no longer exists. Federal payments to provinces are illustrated by a table of figures dating from 1960—before the rapid expansion of the provinces and their challenge to the federal government. Mallory refers to the 1926 edition of R. A. Mackay's *The Unreformed Senate of Canada* but not to the revised edition of 1963. The limitations of both form and content are more apparent since the publication of a third text, Van Loon and Whittington's *The Canadian Political System,* which uses a system analysis framework and provides more than 250 pages on the political process. In contrast to Dawson-Ward's five pages of bibliography and Van Loon-Whittington's 49 pages, Mallory has none at all. Unlike its rivals, the Mallory text does not include the British North America Act as an appendix.

Nevertheless, from his vantage point at McGill University, Montreal, Mallory does offer some guidance and many words of wisdom on what is Canada's most crucial problem today—the nature of Confederation. He notes the difficulty of adapting Quebec's political style, with its emphasis on personal leadership by the Premier, to a truly parliamentary and cabinet system (though recent changes outside Quebec make the contrast less evident than it used to be). He appears to recognize the paradox of parliamentary sovereignty as a principle of government in a federal state—a problem the British are already pondering as they plan to enter Europe. European political scientists interested in the management of a multicultural and multinational community may find much that concerns them in Canada. But, equally, Canadian scholars seeking some alternative to the stark choice between the British North America Act and a separate Quebec may look with increasing curiosity at the European Community. It is Professor Mallory's contention that "an understanding of the constitutional and political arrangements which govern how power is exercised is essential if one is to understand how a country came to exist and the style of its politics" (p. xi). For those interested in political institutions, the congress of the International Political Science Association to be held in Montreal in August 1973 could be an opportunity for instructive comparative analysis.

In sum, if the book is not always in the contemporary idiom, it is nevertheless a scholarly contribution, indispensable for teachers and useful for graduates and senior undergraduates who need an alternative interpretation of Canadian government.

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The Nigerian Army 1956–1966. By Norman J. Miners. (New York: Barnes and Noble, 1971. Pp. 290. $5.00.)

Among the numerous rationalizations for military intervention is the argument that a commanding officer, self-installed in the presidential palace, will take decisive, corrective steps against political ills. As the *miles ex machina*, the "man on horseback" will cure problems by his willingness to resolve nagging problems. And, if I may continue to mix metaphors, he might (in the words of a Nigerian newspaper columnist) "take the bull by the scruff of the neck and grip its horns . . . Those who will not move with the times should be dragged, kicking and screaming—but dragged all the same . . . To the battlements, my general." What a prescription for disaster! *The Nigerian Army 1956–1966* shows how a rush to the battlements can contradict a military's heritage and can lead to increased fragmentation and political woes, the results of military activism in the absence of adequate political information and awareness.

Miners has written a concise political and social history of the Nigerian army as it moved from relative political inactivity in the colonial period to intense political involvement during 1966. The Nigerian army prior to 1958, according to Miners, came "very near" the theoretical ideal of the apolitical army, "an efficient agent to carry out the ends decreed by its political masters without sentiment or complaint" (p. 100). The apparently tight control over Nigeria's armed forces rested upon factors unlikely to recur in contemporary Africa. Expatriate officers retained the reins of command. The men in the ranks were drawn from areas untouched by partisan politics. Politically conscious officers (usually from the South) were isolated in the technical services, or their impact was diminished by mixing with essentially apolitical northerners. Battalions were shifted regularly within the federation, thus diminishing local ties. From 1960 to 1964, significant parts of the Nigerian army were assigned to responsibilities elsewhere in Africa. And, perhaps most significant, few ethnic tensions surfaced. Great Britain continued to furnish the Nigerian army its organizational framework, codes of professional conduct, patterns and centers of training, equipment, and (until early 1965) a decreasing portion of the officer corps. But these essentially external influences waned, opening the army to influences from the society it supposedly served and represented. The diminishing influence of the British, coupled with and magnified by the political chicanery of Nigeria in the mid-1960s, helped make the twin coups of 1966 possible.

Each contestant in the Nigerian game of politics, Miners comments, may have been prepared "to destroy the political system rather than accept less than the fulfillment of his demands" (p. 221). During the period under review, Nigerian politics was characterized first by the increasing significance of, then by the dominance of, the Northern Region, itself larger than the three other regions combined. Those who controlled the North, and hence the federation as a whole, were prepared to make tactical concessions to their opponents *only* in the framework of continued Northern dominance; according to Miners, when this dominance was threatened, "opposition was ruthlessly crushed by force."

Northern political preeminence did not draw the army as a whole away from its apolitical heritage. But as other national institutions (for example, the railway system and the universities) became foci of essentially ethnic tensions, the army itself became susceptible to conspiracy and fractionation. Further, by late 1964, the Nigerian army's heavy international involvement (particularly in Tanganyika and the Congo) came to a close; operational responsibilities had "allowed little time or opportunity for conspiracy." But with the electoral clashes and fraud of 1964–5, and with what appeared to be steps further to consolidate Northern control, a nucleus of officers (almost all Ibos) plotted a successful seizure of control. Political awareness on the part of a handful could be translated, in the unsettled conditions of the period, into a coup d'état. And, six months later, the spread of ethnic antagonisms throughout Nigerian society, considerably enhanced by blunderings on the part of the Supreme Military Government, resulted in a second coup d'état. The apolitical heritage of the Nigerian army had been shattered, probably irrevocably.

Only in a postscript does Miners turn to the civil war and its repercussions. The postscript continues the case study approach, which simultaneously strengthens and weakens the book. *The Nigerian Army 1956–1966* provides information accurately, concisely, and fairly. Miners's judgments, though few, are not controversial—with the possible exception of attributing the July 1966 coup, in the main, to General Ironsi's failure to create a Middle Belt state. I can only wish Miners had gone further. He does not draw general hypotheses from his analysis, nor indulge in extended comparisons with military intervention elsewhere. The postscript only touches upon the fascinating and little-studied area of the military's effectiveness in performing political roles. On these counts, Miners comes off second-best to Robin Luckham, whose *The Nigerian Military* appeared a few weeks later. Even at $18.50, Luckham's study belongs on the shelves of *aficionados* of military intervention—and, I must add, Miner's $5.00 paperback should enrich libraries of African affairs. To have written clearly about the tangled events of recent Nigerian history is no mean task; *The Nigerian Army 1956–1966* is no mean book.

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The Cultural Revolution at Peking University. By Victor Nee with Don Layman. (New York: Monthly Review Press, 1969. Pp. 91. $1.95.)

Mao Tse-tung in the later years of his life has become obsessed with the political consciousness of his successors. He fears that his heirs are losing the revolutionary ideals and revolutionary momentum that he and his followers worked so hard to develop. The Cultural Revolution was Mao's effort to halt this trend and ensure that his successors would continue the revolution he had started. Peking University, China's most prestigious university, became the focus of his effort in its early stages. Victor Nee in *The Cultural Revolution at Peking University* vividly traces this movement in the spring and summer of 1966.

As Mr. Nee tells, since the early 1960s Mao has believed with some justification that China's educational system was becoming increasingly elitist. At Peking University, the proportion of students from worker and peasant families had been reduced from more than two-thirds of the student body during the Great Leap Forward to little more than one-third in 1962. Political beliefs became less important than intellectual ability in gaining entrance. Indeed, Peking University president, Lu P'ing, personified what Mao feared: his commitment to the revolution and equality had been weakened by a commitment to education and specialization.

In 1964, prior to the Cultural Revolution, Mao launched a campaign at Peking University to criticize Lu P'ing's administration. But instead of accepting the criticism, Lu P'ing and his associates castigated their critics, a group of young instructors and students. Unable to revolutionize his successors through this relatively mild method, Mao in 1966 sought to do so through a more intensive, full-scale campaign he termed the Cultural Revolution. The group which had been prevented from criticizing Lu P'ing in 1964, launched a more vigorous attack against him in 1966. This time, Lu P'ing resisted by mobilizing the Communist Youth League. A struggle broke out between the two groups. When Mao sided with the group opposing Lu P'ing in early June that spelled the end of Lu P'ing and the Peking Party government which supported him. Despite a counterattack in July, by midsummer the Maoists had pushed the old Party group out and were in control of the university.

Mr. Nee has meticulously described these events. Yet the impression he creates that these events took place spontaneously is somewhat misleading. It is true that some students freely joined the protest against the Party bureaucrats at the university because of tight Party controls and their own feelings of frustration. But that is different from Mr. Nee's suggestion that these events were initiated without outside instigation.

The author dates the start of the Cultural Revolution from the appearance of large posters at the university denouncing the Party administrators, but he fails to indicate where the rebelling students got the paper and printing facilities which were not readily available. Soon after the posters appeared, parades of workers and peasants arrived at the university in support of the rebelling students. It is probable that these activities were carried out at the instigation of Mao's faction and the army. Mr. Nee's book concludes with the rebellious students in charge of the university, but shortly afterward, a more ideologically orthodox group deposed the rebelling group from power.

There is no doubt that the freer atmosphere Mr. Nee describes must have existed at the time when one group of bureaucrats was being overthrown and another group was being established. Yet this does not mean that this process at Peking University was a spontaneous one, autonomously directed by a number of students and faculty. Overthrow of Party authority at the university could only have been carried out if it were backed by Mao and the army. Mr. Nee tells the right story, but sketches in the wrong atmosphere.

MERLE GOLDMAN

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Political Mobilization of the Venezuelan Peasant. By John Duncan Powell. (Cambridge: Harvard University Press, 1971. Pp. 259. $8.50.)

This case study of the Venezuelan peasant movement and its political settings draws on a variety of sources: historical data, official statistics, some parts of the CONVEN survey conducted by Bonilla, Silva, *et al.*, and the author's own interviews with national, state, and local leaders of the *Federación Campesina de Venezuela* (FCV) and with other political informants.

At first blush, Powell seems to present us with the perfect example of a political exchange system in which peasants trade their votes for the benefits they derive from governmental agrarian reform policies. This explanation seems particularly plausible in the Venezuelan case because of two sets of historical circumstances: (1) Prior to the rise of FCV, the traditional bonds of mutual obligation between peasants and landlords had long since been severed as the result of crude attempts to commercialize agricultural exploitations, thus making the peasants available for political organization and action; and (2) a group of professional political brokers, largely of educated middle-class origins, needed the electoral support of workers and peasants. In addition, the urgency of feeding a rapidly swelling urban population (which increased from one-third to two-thirds of the country's inhabitants in less than a generation) may have stimulated attention to the needs of the agricultural sector.

Yet a closer examination of the data throws some doubt upon the exchange thesis. Moreover, Powell's own conceptualizations are not always consistent with it. His main propositions, abstracted from various parts of the study, may be stated as follows:

(1) Peasants stand in a political exchange relationship with power brokers who provide them with land and other benefits in return for votes.

(2) The relationships between the peasants and the political machinery (government and political parties) constitute a "rural problem solving system" which ensures the authorities of political support.

(3) The political brokers are subject to tensions between "authority legitimization" and "interest articulation." The manner in which these tensions are handled determines both the pace of agricultural reform and the destinies of the regime.

(4) The likelihood of agricultural reform—as against those of revolution or stagnation—depends on the time sequence in the occurrence of three historical processes: commercialization of agriculture, politicization of the rural masses, and urbanization.

A strict interpretation of the *exchange* thesis would lead to the prediction that peasant votes are a function, over time or space, of the amounts of satisfactions which the rural masses actually derive from government benefits. There are no data to indicate this. The local peasant leaders in Powell's sample tend to evaluate the FCV positively, but they react negatively to the political parties and even more so to various aspects of the agrarian reform program (p. 174). To what extent they reflect the feelings of their constituencies is, of course, not known. No attempt was made to show that votes for *Acción Democrática* (AD) or other reform-minded parties came from areas that have been particularly favored by agrarian reform policies.

The obverse of the exchange hypothesis—that the geographical distribution of benefits depends on the strength of party votes—is explored in a scantily reported (and highly questionable) regression analysis which uses the twenty-two federal states as data points (pp. 167 ff.). In the text, the author mentions some positive correlations occurring in otherwise unreported regression equations between unnamed benefits and AD as well as Christian-Social votes. But the regression equations actually presented in table form shows *negative* regression coefficients for party votes. To say the least, the question at issue remains unsettled. It would appear that benefits have gone chiefly into states in which the value of agricultural production is high and that FCV membership strength (but not the party vote) partially determines the flow of benefits.

To characterize the relationships among FCV, political parties, and peasants as a *"rural problem solving system"* is to strain the data. The study does not demonstrate that problems are actually being solved. It shows, however, that the FCV communications network deeply penetrates the rural areas and that its channels may carry "negative feedbacks" (i.e., complaints) which the agricultural administrative system does not readily transmit. The political leadership is accessible, but there is room for questions as to its responsiveness. Most local peasant leaders say that government agencies and programs are of "little" or "no help"—and the proportion of critics is highest among those leaders who have either the largest or the smallest amounts of contact with official agencies. Much of this suggests that peasant attachment to the regime may be due to the psychic rewards of participation and to the peasants' traditional dependency needs (Powell alludes to these on pp. 219–20)—not necessarily or exclusively to the regime's ability to solve rural problems.

The government's deficiencies in satisfying rural needs are due, in the author's words, to the "tensions between legitimacy and interest articulation." The use of these modish terms is unfortunate, since it obscures more than it illuminates. What is actually at stake is the pace of agricultural reform, the government's condemnation of land invasions by landless peasants, and its concentration on the settlement of unused lands rather than the breakup of *latifundia*. In principle, there is no reason to believe that such a law-and-order position coupled with respect for property rights would necessarily make a government more legitimate than a vigorous policy of property redistribution. Actually, "legitimacy" here stands for the support of the urban groups whose apprehensions of governmental radicalism must be allayed lest they repeat their performance of 1948, allying themselves with the military in overthrowing the regime. The use of the static concepts of "legitimacy" and "interest articulation" forecloses an adequate description of the political dynamics of agricultural reform.

Powell's last proposition, his contribution to a general theory of agrarian politics in developing nations, is unrelated to the previously discussed tradeoff mechanisms—i.e., of votes against benefits, and of legitimacy against interest articulation. It is more akin to the notions of such writers as Hagen and Huntington which rest upon some version of the frustration-aggression model. According to Powell, agrarian reform is most likely if commercialization of agriculture precedes the politicization of the rural masses, and both of these precede urbanization. If the rural masses become politicized prior to commercialization, this creates a revolutionary situation—evidently because of the high level of peasant frustration. This potentially fruitful hypothesis, amplifying previous theories, obviously could not be tested within the framework of a case study.

Indeed, it is perhaps too much to expect of a case study—as I have done implicitly—that it should serve as the crucial test pitting against one another two competing types of theories: "economic" and "psychological" ones. Perhaps no crucial test is possible under any circumstances, because the mechanisms postulated by the two types of theories may both operate in the same situation. If this be the case, one would wish to specify the conditions under which either of the two types of political processes is likely to occur. But this cannot be done in a case study.

Powell has assembled a significant body of data and presented ingenious, although in my view doubtful, interpretations. The book's failings are not entirely his. They proceed in part from the inchoate state of the art and in part from the cussedness of the real world. The most exigent critic would have to call this study stimulating.

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Maoism in India. By Mohan Ram. (Delhi: Vikas Publications, 1971. Pp. 196. Rs. 22.50.)

The impact of the Sino-Soviet controversy on the communist movement in India is the subject of this timely study. The author, a political journalist in New Delhi, argues that in the eyes of the two major communist powers, the USSR and the Peoples' Republic of China, India is a test case for their respective views on the prospects of peaceful transition to socialism or the necessity and appropriateness of rural-based insurgency as the path to communism in the Third World. As a result, long-standing tensions within the Communist Party of India (CPI) have been exacerbated and the Indian communist movement fragmented. Mr. Ram traces the emergence of three communist groups capable of making a significant challenge to the pro-Moscow CPI: the Communist Party of India—Marxist (CPM), the Communist Party of India—Marxist-Leninist (CPML), and the Andhra Pradesh Revolutionary Communist Committee (APRCC).

Seeking to combine constitutional and extra-constitutional forms of struggle, the CPM participated in the coalition governments of West Bengal and Kerala in the late 1960s. The Naxalbari uprising in the strategic Darjeeling District of West Bengal forced the CPM to choose between supporting unlawful seizures of land and preserving the alliance with its coalition partners, thus exposing the inherent contradictions of its position. When the CPM pursued the latter course, dissident communists under the leadership of Charu Mazumdar formed the more radical CPML, which has the distinction of being the only communist group in India recognized by the Communist Party of China. The other important Maoist organization in India, the APRCC or Nagi Reddy group, has refused so far to join the CPML and continues to follow an independent line.

Although there is some overlap with the author's previous study, *Indian Communism: Split Within a Split* (Delhi: Vikas Publications, 1969), the present work presents new material on the conflict between various Maoist groups in India over revolutionary strategy and tactics. These differences are brought out clearly in the context of analyses of the Srikakulam and Warangal agitations in Andhra Pradesh and events in West Bengal. The CPML, operating underground, has conducted a terrorist campaign against individual representatives of the feudal order in the countryside and, more recently, of the state machinery in the cities. Opposing the Mazumdar line of physical annihilation of landlords and policemen by small, armed guerrilla detachments, the APRCC emphasizes mass participation, the relationship between violence and the construction of a mass base, and the formation of both a legal and an illegal apparatus.

The author agrees with the assessment of the Chinese communists that India is ripe for a Maoist-type revolution but doubts if the various communist groups are capable of coordinating their activities in the pursuit of correct policies (pp. 187–188). Given the segmental nature of Indian society, however, it is unlikely that a successsful revolutionary movement on an all-India scale could be launched even if these groups were able somehow to overcome their differences. Contrary to the author's contention, Naxalbari, where a significant uprising occurred in 1967, is not "a microcosm of semi-feudal India" (p. 38). More than 50 per cent of the population of that area is composed of Scheduled Tribes, and another 25 per cent consists of members of Scheduled Castes. The presence of large tea plantations and the widespread use of tribal cultivation has contributed to a history of land disputes unparalleled in the rest of India. It should be noted that in Srikakulam and Warangal as well there are unusually large tribal populations. One wonders if such areas could possibly serve as catalysts for class-based revolution throughout India.

The author does not question the applicability of the Maoist model to Indian conditions. The very real differences between revolutionary China and contemporary India are ignored. In

China the communists were up against a nationalist movement with little mass support, no sympathy for agrarian reform, and dependent upon feudal warlords for survival. In addition, during the crucial years of the Chinese revolution, the attention of the major powers was diverted to a struggle for their very survival. None of these conditions are to be found in India today. The author points to the poor showing of the Congress party in the 1967 elections. Although the party did suffer some severe reverses, its vote did not drop to 37.87 per cent as the author claims (p. 50), but to 40.73 per cent in elections to the national parliament and to 39.96 per cent for State Assembly elections, according to official reports of the Election Commission. Before this election, the Congress party had never polled more than 47.78 per cent of the vote in national elections. Moreover the party's fortunes have since revived under the dynamic leadership of Indira Gandhi. Even if the central government should weaken in the future, India's conservative parties, the military, and the civil service would still block the path to power.

Since the study is based largely on a textual analysis of such products of the communist press as newspapers, journals, and party documents, it is not surprising that the resulting treatment of communist ideas should be somewhat detached from immediate political realities. A full account of the Indian communist movement would require extensive interviews with its leading figures and the placing of the problem of communist revolution more firmly in the context of political, economic, and social change in India. Until we have such an account, the present study will serve as a good short introduction to some of the major problems facing Indian communists today.

John Patrick Haithcox

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Partners in Development. John M. Richardson, Jr. (East Lansing: Michigan State University Press, 1969. Pp. 272. $8.50.)

The subject of AID-university relations has been studied many times over the last twenty years. Another book on this familiar topic may seem unnecessary and even superfluous, but Mr. Richardson has brought together both a depth of data and a level of analysis that have been lacking in most previous accounts. Therefore, it is a welcome volume, and a contribution to the literature of the field. In addition, it could serve to promote more effective operations in relations of this kind in the years ahead.

The study was undertaken as a part of the CIC-AID Rural Development Research Project. It purports to do three things: (1) to review chronologically the evolution of AID-university relations from 1950 to 1966; (2) to analyze systematically the several elements of stability and change in agency-university relations; and (3) to present a theoretical framework or theory of administrative behavior, on the basis of which the analysis was made.

Mr. Richardson has been quite successful in his first objective. In fact, the 138 pages devoted to the history of agency-university relations constitute the most concise and accurate such account to date. An impressive number of interviews was held with key figures in AID and its predecessor agencies, together with a representative group of university officials and representatives of university associations. Many of the interviews were conducted in depth, and appropriate documents were consulted. While still more interviews would have helped and additional documents and reports might have been consulted or at least mentioned, the research effort does seem adequate for the purpose of a summary history.

One of the strengths of the volume lies in the author's attention to the underlying goals of the organizational participants in the AID-university drama. Richardson outlines the varying aims of the agency under a succession of administrations and agency heads. He also sets forth the hopes and objectives of the universities and university associations fairly realistically. All of this, however, is examined in a restricted context of AID-university relations that tends to exclude program objectives.

The somewhat briefer section that analyzes the factors that make for stability and change (real "peace and harmony") between agency and institution is both realistic and perceptive from a "how to win friends" point of view. It is here that present and future administrators will find useful data and conclusions about enterprises that require cooperation among donors and contractors. The author has identified and discussed most of the salient factors in donor-contractor relations at home in regard to projects carried on abroad. His analysis has depth as well as clarity.

The theoretical framework underlying the analysis is presented in an appendix—a suitable portion, given those who are likely to make up the audience for this book. There is little that is unusual in the presentation, particularly for admirers of Herbert Simon. Still, a persuasive case is made for a satisficing theory rather than a more limited rationalistic model.

Nevertheless, there is one great shortcoming to this volume. It is that the book and its analysis are based upon a rather simple-minded Dale Carnegie approach to human relations. If your neighbors and associates like you, it is good. If they dislike you, it is bad. This is the kind of concern that caused so much difficulty in the entire agency-university controversy over the years: a consuming focus on the nature of AID-university relations, and substantial inattention to the impact achieved in the field. If there were no logical connection between the two, the entire matter could be dismissed as an author's preference. But agency-university relations exist in order to facilitate impact in the field. Under what set of relations does the best field situation obtain? Do peace and harmony at home bring forthright and effective action abroad? Does some measure of controversy actually enhance a project's prospects for substantive success? Indeed, is it even possible to have warm and harmonious relations in an area as complex as AID-university relations? Does human intercourse in complex organizational situations preclude the achievement of consensus? Are consensus and contentment, indeed, the hallmarks of good governmental administration? One searches in vain for answers to questions such as these. The questions themselves are not posed for the most part.

Within the confines of a narrow approach to human relations in a complex organization, the author of *Partners in Development* has given us a high-quality monograph. It is balanced, careful in detail, and replete with insight. It should bring to an end the studies of AID-university relationships, short of focusing on the impact of the projects abroad and the impact of the projects at the home campuses of American universities.

EDWARD W. WEIDNER

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The Politics of New States: A General Analysis with Case Studies from Eastern Asia. Edited by Roger Scott. (George Allen and Unwin, 1970. Pp. 201. £2.10.)

This collective enterprise takes the form of a number of essays (completed early in 1969) designed to introduce Australian undergraduates to a growing field of academic inquiry. Although the blurb on the dust-jacket represents this book as satisfying "the special need for a comprehensive course of study," the editor with becoming modesty views the endeavor as "a selective study." To this end a general introduction explaining the nature and problems of politics in new states is followed by a number of chapters whose prime unifying link is the relevance of the countries to Australian interests.

In many ways this is an unexceptional book. The editor provides the reader with an elementary but useful survey of the principal characteristics of new states and deals competently in the process with the relevance of conventional concepts, the significance of precolonial influences, the experience of decolonization, the problems of political and national integration and the failure of postindependence political institutions. In all this, there is perhaps a tendency to overgeneralize from African experience—given the geographic orientation of the case studies—but in the main the introduction matches the terms of reference set by its author. What follows is no less unexceptionable though perhaps somewhat less useful for the student constituency for whom this work is intended. Chapters on Japan and China examine the course of critical political change, though in the case of the former state the survey ends in 1947. There follows a general inquiry into the position of the Overseas Chinese which poses the problem in terms of absorption, succeeded by two studies of social and political change in Vietnam and Indonesia in which the former chapter by Peter King is the more worthy synthesis. Finally, the relevance of African experience is applied to the political condition and prospects of Australian administered New Guinea. With the possible exception of this last chapter, it is doubtful if students will find anything in this collection which is not available in more original and stimulating form elsewhere. However, providing they are not so complacent as to conclude that all their intellectual needs in this field will be satisfied within this set of hard covers, this volume is not without merit as an introductory text.

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Ibo Politics: The Role of Ethnic Unions in Eastern Nigeria. By Audrey S. Smock. (Cambridge: Harvard University Press, 1971. Pp. 274. $10.00.)

Professor Smock's book, *Ibo Politics,* provides insight into one of the more bedeviling aspects of the role of ethnicity in the politics of Africa. The Ibo, an acephalous, decentralized people who live in the eastern regions of Nigeria, became the most ardent advocates of a strong Nigerian central government during the 1950s and the early 1960s. Yet, in the late 1960s, it was the Ibo who led the Biafran secession. Thus, the most pan-Nigerian of Nigeria's peoples became the most inward looking.

The Ibo case illustrates that adaptations to "modernization" in Nigeria seemed more to strengthen than to weaken ethnic affiliation. For somewhat complex cultural reasons not adequately dealt with in this work, ethnic identity assumed new dimensions as members of the Ibo community outside Eastern Nigeria distinguished themselves from others among whom they lived. Wherever they went, the Ibo established town and clan improvement associations. The role of these unions in the developing political system of eastern Nigeria is the topic of Ms. Smock's book.

In contrast to other ostensibly similar organizations in Africa, Smock tells us, Ibo ethnic clan improvement unions were rurally oriented and existed for the purpose of channeling resources to and developing the rural, home communities. The ethnic unions described by Smock evolved as modern organizational vehicles which would transform the countryside. These indigenously inspired unions flourished while the institutions of representative local government, created by the British, languished. In this volume, two very different unions, the Abiriba Communal Improvement Union and the Mbaise Federal Union, are studied in two locales: in their rural, home base and in Eastern Nigeria's leading urban and commercial city, Port Harcourt—yielding four case studies. The functioning of these unions is evaluated in three issue areas—community development, education and nomination for political office. Based on these four case studies, an attempt is made to generalize the experience of the Ibo ethnic unions in the context of a modernizing state in terms of three indices of political development: institutionalization of the administrative and political structures, political integration, and modernization of cultural attitudes toward the political system.

Smock's implication that a high degree of institutionalization and organization may actually be dysfunctional for African political parties is most interesting and deserves fuller investigation. The low level of institutionalization as defined by western political science may have been most functional to politics in Eastern Nigeria for it permitted the dominant party to absorb and accommodate traditional political fragmentation by allowing the subgroups, represented by the unions, to regroup and realign themselves within the party as they competed for scarce economic resources. In addition to the party, Smock studies political institutionalization by looking at the connection between the local government councils and the ethnic unions. The ineffectiveness of the urban council in Port Harcourt in meeting overwhelmingly complex urban problems, together with the impressive effectiveness of the ethnic unions in reinforcing rural ties in an urban setting, militated against a transferal of identity away from the primary group. Consequently, Ibo society was able to cohere in the midst of rapid change. In the city, the subgroups provided a focus of identity and reinforced traditional loyalties. In addition, the unions significantly contributed to the standard of living in the countryside as they diverted resources from consumption in the urban areas to community development in the rural areas. The potential for rapid development in a decentralized, highly particularistic society is most intriguing and raises important questions about assumptions which currently underlie analyses of both political and economic development.

The question of whether ethnic unions contributed to or detracted from political integration in Eastern Nigeria is most controversial. Ms. Smock theorizes that national integration does not necessarily presuppose a displacement of existing commitments, but rather that subcultures can exist and contribute to a common nationality so long as culturally diverse people agree that they constitute one nation. In light of the vicious Nigerian civil war, however, this argument seems somewhat specious.

Smock's parallels between ethnic groups in the United States and Ibo subgroups are original, as are her distinctions between political cooperation and cultural assimilation. But, while it is true that cultural differences need not necessarily thwart political cooperation if the system offers incentives to foster such cooperation, one must know how this cooperation is maintained and what kinds of incentives are necessary to foster such cooperation. Although Smock's suggestion that ethnic competition is a modern phenomenon seems valid, the crucial question is how to contain and make tolerable such competition. Fundamental agreement regarding "the rules of the game" was absent in Nigeria. The question then is to determine how crucial ethnic diversity was to the replacement of competition by war between the peoples of Nigeria. Ethnic unions in themselves do not preclude a widening of national identity, but the cultural norms reinforced by the unions may be incompatible with those of other groups forced by colonial fiat into the same political community. Cultures may become more mutually exclusive as conflicting values clash.

The role of the unions in cultural modernization illustrates many of the contradictions inherent in modernization theory. Ibo traditional society was egalitarian, achievement-oriented and participant, pragmatic, innovative, and instrumental in its approach to politics. The ethnic unions were organizations which bridged the psychological and political gap between rural and urban areas and prevented the discontinuities in the political system which have led to what Samuel Huntington has called "the green uprising."

The book contributes essential knowledge to our understanding of political development and shows how diverse the path may be. This particular study, however, reads somewhat like a doctoral dissertation. Many details (such as anthropological background, oral traditions of origin, notes on methodology, names of various scholarship winners, etc.,) are important to the rare scholar interested in Abiriba and Mbaise but are not necessary to the general thrust of the text; these details would better have been relegated to the footnotes. And, material on other voluntary associations in other areas is somewhat irrelevant since these associations were different in both form and function from the Ibo unions. The attempt to follow rather rigidly the standard models (Almond and Powell, Huntington, Dahl) results in some distortion and much repetition of material. In addition, there are a few errors of interpretation of both oral tradition and earlier sources, but on the whole the book in an important contribution to the literature on political development.

BARBARA CALLAWAY

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Czechoslovakia Since World War II. By Tad Szulc. (New York: The Viking Press, 1971. Pp. 503. $14.00.)

This book by the veteran *New York Times* correspondent is a well-written account of the history of Czechoslovakia since 1945. Although the author is well versed in his subject, the book makes no pretensions to be scholarly. It is, moreover, somewhat uneven, since the events of 1968 occupy more than half of the book. Szulc correctly assesses that the geographical position of Czechoslovakia has been the major factor in her political difficulties with avaricious neighbors. In 1945 the Red Army occupied Czechoslovakia, and under its tutelage the strong Czech communist party embarked on the slow road of subversion which would culminate in the coup of 1948. Twenty years of communist misrule, the bloody purges of the early 'fifties, the economic stagnations, and the unwillingness of the communist boss Novotny to initiate reforms or de-Stalinization culminated in the events of 1968. When Novotny was finally forced to step aside, his place was taken by the weak and indecisive Dubček. It seems in the final analysis that the near anarchy in Czechoslovakia and Dubček's unwillingness or inability to control the more radical and anti-Soviet elements finally tipped the scales in favor of those Russian leaders who preferred military intervention. The Soviet Union apparently would tolerate a dissident and totalitarian Rumania but could not afford a democratic Czechoslovakia.

To its credit the Czech revolution was free of the brutalities of the 1956 Hungarian revolution, and its repression gave rise neither to mass executions nor to emigration of the revolutionaries. The Russian tanks crushed the recently acquired freedoms, but there were no Lidices or My Lais. The Czech working class was no more aroused than its fellow hardhats in this country were over the Cambodian invasion. Some of them, of course, had seen the same scenario before and had remained to work faithfully for the Nazi war machine. The conditions may seem bleak now, but the Czechs have suffered before under foreign invaders, and Husák, a victim of Stalinist purges himself, may yet prove to be another Janos Kadar. Szulc occasionally gets carried away and makes categorical statements which could easily be challenged, e.g., his classification of Soviet delegates into neat categories (p. 319 and p. 353). There are also some minor errors. Konev and Rokossovskii did not serve in the Spanish Civil War; Svoboda's forces were not attached to Konev's but were part of the much weaker Eremenko's 4th Ukraine Army Group. As a result, the honor of the liberation of Prague, one of the strangest episodes of the Second World War, had to be shared by the long dormant Czech resistance, Vlasov's ragtag army, and the forces of two ambitious Russian generals diverted south after the Berlin operation. Szulc also incorrectly states that the Jews were particularly singled out in the Polish purges of the early 'fifties; at that time some of the foremost Stalinists in the Polish communist party and secret police were Jewish, and the violent anti-Semitic campaigns in Poland did not begin until the 'sixties. They became especially intense after the 1967 events in the Middle East, at the time when the "nationalists" were in control in Poland.

Although aimed at a general audience, the book is a useful addition to any reading list for courses on East European history and government. For those who want to go beyond Szulc there exists a massive literature compiled in *The 1968 Czechoslovak Crisis: A Bibliography 1968–1970.* (American Bibliographic Center, 1971)

MICHAEL PARRISH

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The Brutal Takeover. By Kurt von Schuschnigg. Translated by Richard Barry. (New York: Atheneum, 1971. Pp. 383. $12.50.)

Kurt von Schuschnigg became Chancellor of Austria in 1934, following the murder of Engelbert Dollfuss in an attempted Nazi coup. Heir to the Dollfuss authoritarian state, he faced mounting German pressure in the following years. As Austrian independence was increasingly threatened, Schuschnigg sought to postpone the seemingly inevitable *Anschluss*. In March, 1938, he tried a plebiscite as a last ditch effort, only to precipitate German action and bring about his own resignation. Within hours, German troops crossed the frontier and Adolf Hitler welcomed seven million Austrians "home into the Reich." A plebiscite of a different sort was carried out, and Schuschnigg was arrested and imprisoned. Released by the Allies after World War II, he traveled to the United States, where he accepted a position on the faculty of St. Louis University. Schuschnigg retired in 1967 and returned to his native country, now a democratic republic. He has written *The Brutal Takeover* about Austria and *Anschluss*.

Opening with a few reminiscences of 1919 and 1945, *The Brutal Takeover* quickly moves to a general survey of some of the difficulties besetting the Austrian Republic after World War I. Economic viability was an issue for the new state then, and Schuschnigg concludes that fears about viability stimulated *Anschluss* sentiment. Nevertheless, he points out that the question of viability has been differently answered since 1945, and the first steps towards future independence had already been taken in the 1920s.

After discussing sympathies for *Anschluss* in the post-World War I republic, Schuschnigg turns to a review of the political divisions among the Austrian people. Gliding lightly over the Christian Social Party, Schuschnigg has more to say about the Heimwehr. He points out that this organization was nonfascist in its early days, and narrates briefly its transition through the 1920s and the role of Ernst Rüdiger von Starhemberg. But he has not intended this description to be analytical or searching. He then turns to the Social Democrats—the nub of a difficult problem for Schuschnigg, for the party was legal during the republic, but was suppressed after the civil war and during the years of his chancellorship. Critics of his government have contended that he could have cooperated with the Marxists against the Nazis. Schuschnigg concludes that the Social Democrats, and particularly the chief party theoretician, Otto Bauer, have been much misunderstood. Like the Christian Socials, they were aiming for an electoral majority, which they expected to achieve democratically. Nevertheless, their leaders had not abandoned the idea of a social revolution and the concept of dictatorship of the proletariat.

The former chancellor does not believe that the Social Democrats had a chance to achieve democratic power in the 1930s. He cites election results to show that they hovered at about 42 per cent of the vote by 1931 and declined after that. Although Schuschnigg mentions that the Christian Socials declined further and that the old German Nationalists were being wiped out, he does not believe that the socialists would have been able to emerge as the largest party in the republic. Many Austrian Social Democrats, however, hoped for national elections after 1933 and confidently expected to achieve a commanding plurality—enough to enable them to occupy the driver's seat of a coalition government. Schuschnigg, however, agrees with the official Dollfuss position that conditions in Austria after 1933 would not permit any free national elections. Thus, the Social Democrats had to accept the authoritarian direction of the state. He has little to say to them after 1934, and nothing to add about the prospects for coalition against the Nazis.

The main body of the book is the former chancellor's account of Nazi pressures and Austrian response after 1934. Schuschnigg treats this in a topical narrative of remembrances, interspersed with references mostly culled from readily available materials and replies to some of his critics. The result is not much beyond other works or his own previously published books.

The failure of Great Britain, France, and Italy to agree to a defense of Austrian independence left Schuschnigg's government to its own devices. Hitler steadily eroded Schuschnigg's freedom of action as the chancellor of a second German state. Schuschnigg argues that not all who favored *Anschluss* then were Nazis; the enemy was not Germany but Nazism. He agrees that the decision to hold a plebiscite triggered final German action, and although admitting that the move was approved on false premises, he concludes that the results were "the lesser of two evils for Austria," since the "assimilation" was thus allowed to appear less voluntary.

There were no prospects of resistance, according to Schuschnigg. He has little to say here about the Social Democrats, and asserts that the Austrian Army might have held out for two days, at best. Only five per cent of the officer corps may have been Nazi, and the army was loyal, he claims, but his government expected that 35 per cent of the population would have voted "no" in his own plebiscite. Still, military resistance would have had no purpose, not even symbolic, and foreign intervention was out of the question.

Schuschnigg loves his country, and is undeniably a patriot, but he lacks the hardness and cold reflection that could permit him to view these critical events and his own role with detachment. He has come to believe that the Austrian people have grown closer together and that the former divisions are gradually becoming less harsh and severe. This is indeed illustrative of the changes that have taken place all over central Europe in the past three decades. One would wish, however, that Schuschnigg had delved deeper into the period of his government and provided more incisive and detailed comments on the final years and months. This would have been more useful.

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Search for Peace in the Middle East. Prepared for the American Friends Service Committee. (Hill & Wang, Inc., 1971. Pp. 126. $4.50.)

After reading this book for the first time—and the second and the third—I was so impressed that I decided to organize a lecture to a group of Oxford students around it. It was only after I heard myself trying to explain its ideas to wise young minds that I recognized them as no more than the same old trusting, unrealistic wishful thoughts that all of us old Middle East hands have engaged in at one time or another, except that they have been given special force by the sincerity and patent goodwill of the writers. It is no wonder that State Department officials reading it from the safe distance of Washington regard it as "the most sensible and constructive set of recommendations yet to appear on the Arab-Israel impasse."

The writers, a working party of Quakers headed by Landrum Bolling, President of Earlham College, are among the most intelligent, most articulate and, in a way, most knowledgeable yet to approach this subject. But, for all their qualifications, their opinions must be judged as being those of "private persons with no special political or military intelligence sources," to use their own description of themselves. They see American, Soviet, Arab, and Israeli viewpoints almost exactly as the American, Soviet, Arab, and Israeli governments intend for them to be seen. They take official statements of these governments at more or less face value, forgetting that governments make statements not merely to convey information or to expose true sentiments but to achieve practical ends. ("In examining an official statement," says an old CIA manual, "don't ask yourself what it means; ask why it was made. Look for motive, not meaning.")

For example, members of the working party had highly intelligent, and potentially revealing talks with Soviet officials in Moscow, at their U.N. Mission in New York, and in various embassies in the Middle East, and afterwards drafted a summary of Soviet policy which is as tightly reasoned and well phrased as the Soviet Foreign Office could have drafted for itself. And it is so "right," its arguments so airtight, that one wonders how it is that the Arabs have any reservations at all in their siding with the Soviets against the West. Moreover, after interviews with Western official observers, the writers concluded that the American and British governments accept the Soviet position pretty much as the Soviets explain it. It happens that the writers are wrong on both counts.

The principal question dangling between observers who are unable to peek behind the scenes and those who spend their working lives there is this: Do the Soviets want war, or peace, or a prolonged no-war-no-peace situation? The writers of *Search for Peace in the Middle East* argue (p. 65) that "in their long range interests Soviet leaders have a genuine desire to see a political settlement along the lines of U.N. Resolution 242" (that they want peace, in other words), and for two reasons. First, "they fear that they cannot control indefinitely a no-peace-and-not-quite-all-out-war situation" (p. 65). Second, the Soviets no longer need the threat of war to maintain their position in the Arab world. But Western government officials who live with the problem tend to an opposite belief: first, that the Soviets know very well that they couldn't goad the Arab governments into war with Israel even if they were misguided enouh to want it; second, that the Soviets realize that in competition with the West in conditions of total peace they would lose their position in a matter of weeks.

It happens that the Arabs are fundamentally anticommunist and, at the same time, their leaders are sensible enough to see that economic involvements with the Soviets have dangers that involvements with the West do not, and without the advantages. There is evidence that the Soviets regard Arab military leaders as being, if anything, not hawkish enough ("much bravado in their posturing, but no will to win," as a Pravda article put it), and more evidence that Soviet ambassadors in Arab capitals are in fact giving the various governments the same kind of provocative misinformation that brought the Egyptians and the Syrians to the brink in 1967. There are also indications that the Soviets believe that the Arabs' fear of an Israeli attack is virtually the *only* factor currently working in their favor. An Arab attack on Israel, as they see it—and as our *own* officials see it—simply isn't in the cards. They can therefore safely encourage a bit more hawkishness on the Arab side.

The authors cannot be blamed for misjudging Soviet views—since, after all, they wrote their book almost two years ago and the signs of frictions between the Soviets and the various Arab governments are only now coming to light. But they really shouldn't have allowed themselves the faulty thinking implicit in such phrases as, "Egypt wants peace" or even "the Egyptians want peace" or "The Arabs want peace." "Egypt" is a country, and can't "want" anything. "The Egyptians" are a people out of reach of public opinion polls, and what they want cannot be determined. "Arabs" live in a number of different countries and, judging by their actions, their main disputes are with each other, not with Israel. What is important in any Arab country is what its leaders want. And what they want is to stay in power. It takes but a little imagination to see which set of conditions best favor their chances—war, peace, or no-war-no-peace.

All the same, with the omission of the italicized paragraphs beginning with such phrases as *"It is the considered opinion of the authors that . . . ,"* this book is the most concisely informative, most clearly written, and generally most valuable explanation of the background, the issues, and the stands of the participants—in less than a hundred pages far superior to almost all of the 300-odd page back-breakers to be found on the subject.

MILES COPELAND

London

America's Response to China. An Interpretative History of Sino-American Relations. By Warren I. Cohen. (New York: John Wiley & Sons, Inc., 1971. Pp. 242. $7.50, cloth; $3.95, paper.)

A fresh look at the history of United States diplomacy toward China is very timely, and Warren I. Cohen supplies it in two hundred finely wrought pages that put the issues in a new Europe-versus-Asia framework so neatly that the subject will never be the same again. With a judicious command of the sources, he explicates the well-known gap between Washington's fine words and meager actions in support of the Chinese nation. The key to American nonperformance in East Asia lay in the fact that no one in Washington ever felt that the area was of vital importance to us, either economically or strategically. We went to war in East Asia the first time only when Japan had joined up with the European menace of Hitler's Germany. Later, we tolerated the Communist takeover of China and went to war again in 1950 only when North Korea became an arm of the European menace of Stalinist Russia. Europe we considered of "vital interest" to us; East Asia could become so only when joined to Europe.

Given this framework of motivation, our "great aberration" began in 1950 when the Korean war to stop Soviet-managed aggression led us to oppose the rise of Asian nationalisms in China and Vietnam on the grounds that they were ideologically communist. We can see now that we fearfully attached too much importance to mere ideology, for it alone cannot dominate power politics.

A review can hardly do justice to the multiplicity of insights and reappraisals in this very readable volume. Mr. Cohen begins by bringing the American onto the Chinese scene and into the British-created system of treaty privileges. (One may suggest that while these privileges subjugated China on paper, the Chinese often found out how to blunt or exploit them in self-defense.) Instead of equal privileges in the United States, Chinese immigrants met racist violence and eventual exclusion. The inexpensive rhetoric of the Open Door doctrine ushered in the heyday of missionary philanthropy in China, but neither one made China into a vital American concern. After 1900 American policy variously acquiesced in or opposed the rise of Japan, but in any case, Japan arose. The efforts of the Washington Conference in 1922 to internationalize China's problem of Japanese encroachment collapsed in Manchuria after 1931.

In short, the Americans' self-image as "champions of the sovereign rights of China," though reasserted wilfully and often, seldom led to action. While Japan ravaged China, FDR appeased the Japanese militarists, until they unwisely joined up with the Nazis and threatened our vital interest in the fate of Europe. The result, as Mr. Cohen points out, was confusing. By our war with Japan, "China was transformed into an 'ally,' not a real ally, but a symbolic ally, as she was transformed into one of the 'democracies,' but not a real one" (p. 162).

Mr. Cohen takes account of the universalist moral strain in American foreign policy attitudes—the Wilsonian idealism about self-determination, the peace movement for collective security in the 1920s and '30s, the anti-Communist crusade of the 1950s—but his research on policy formation shows that the invocation of universal principles by a Wilson, a Stimson, or a Hull never resulted in the movement of ships and troops to East Asia. Perhaps he would agree that the great aberration after 1950, coinciding with the American acceptance of responsibility as top power, came from a coalescence for the first time of the ideals of universal morality shared by the American public with the power issues of realpolitik seen by policy makers. Until Pearl Harbor, the activity in America's response to China had taken place almost entirely in the great private sector of missions, philanthropy, education, and hopes for trade and investment, not in government channels. This activity had often been revolutionary in its repercussions within China, but it always remained reformist in theory, never contemplating the violence necessary for revolution.

When FDR chose to confirm Hurley's support of Chiang Kai-shek against Mao Tse-tung in 1945, he represented a whole generation of Americans who wished China well in the abstract but thought it a sideshow in power terms, and still hoped "reforms" could obviate revolution. This made Mao more dependent on Stalin, and the Chinese Communists' tentative opening toward American aid, delineated in John Stewart Service's recent monograph, (*The Amerasia Papers: Some Problems in the History of US-China Relations* [Berkeley: Center for Chinese Studies, University of California, 1971]), was closed off. In 1949 Truman and Marshall "acquiesced in the communist conquest of China," because they "did not consider American interests there to be vital" (p. 143). When Washington's myopia let the omnisapient MacArthur head for the Yalu in 1950 and trigger Sino-American hostilities, it was too late to pursue a realistic China policy. "Containment" of Peking came as a Far Eastern extension of the resistance to Soviet expansion in Europe. Support of France in Vietnam also began as a Europe-first policy. The logic of not opposing Asian nationalisms that were bound to come to power anyway, lost out to an excess of ideology, which was a poor substitute for thought. Thereupon, "the Chinese Communists and Chiang's friends in America . . . entered into a symbiotic relationship, feeding off each other's hate" (p. 201).

Warren Cohen's analysis argues for a drastic revision of our world view. It appears that we have been in the grip of our European background more firmly than we realized. If our "vital interest," like love, is where we find it, then the fact that our largest overseas trade is now with Japan, and our two nuclear nonallies —Russia and China—are nearby, should give us a new sense of parity between our Atlantic and Pacific interests.

As a comment on the whole field of American historiography, one may suggest that the search for an American national policy toward China, during the nineteenth-century era of the unequal treaty system there, is a will-o'-the-wisp. United States policy was a by-product of the British policy that fought for, set up, and preserved the system of treaty access and privilege in China. In short, it was only a semipolicy, a most-favored-nation frosting on the real cake of Britain's informal commercial empire in East Asia. American archives are quite inadequate for the study of this nineteenth-century situation, since the policy was made in London; and this no doubt contributed to the feeling in Washington that access to China, a British achievement, was not of vital concern to Americans. The distortion possible from reliance on American sources is illustrated even by Mr. Cohen when he states on pages 56–57, for example, that in 1900 "the United States successfully deflected proposals from the powers to extend operations against the Boxers to other parts of China [than the Peking area]. . . . Thus . . . the possible partition of China [was] forestalled." This is the eagle screaming, quite contrary to the facts.

J. K. FAIRBANK

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Word Politics: Verbal Strategy Among the Superpowers. By Thomas M. Franck and Edward Weisband. (New York: Oxford University Press, 1971. Pp. xiii, 176. $5.95.)

Putting it quite simply, *Word Politics* is a good book. It is brief, well written, interesting, and worthwhile—all in all, a pleasant combination. Thomas M. Franck, Director of the Center for International Studies, New York University, and Edward Weisband, Assistant Director of the same center, have harmoniously blended case studies, logical analysis, theoretical analysis, and policy prescription. Moreover, the context of superpower relationships makes the book quite relevant.

The central thesis of the book is that the rhetoric of nations, especially superpowers, is a crucial part of their posture in international politics. Verbal strategy not only provides short-term justifications for actions in the national interest but also contributes to the management of future crises and the transformation of behavioral norms and expectations in the international system. Thus long- and short-run implications must be considered and the verbal strategy of the United States should be to "listen to ourselves as if we were the enemy speaking."

Czechoslovakia, Cuba, Guatemala and the Dominican Republic are the cases which illustrate a particular type of verbal strategy used by the superpowers. Turning first to the Soviet intervention in Czechoslovakia, the authors point out that the communist superpower felt free to act in an unpredictable, inconsistent fashion within its own regional sphere of influence. Justification for this action was provided by the Brezhnev Doctrine.

Although the United States acted in an appropriately indignant fashion, the authors suggest that the Soviet Union had done no more than follow the principles articulated by the United States in its policies toward the Western Hemisphere. Returning to earlier United States interventions, Franck and Weisband find a mirror image of the Brezhnev Doctrine in the United States' verbal strategy toward Guatemala in 1954, Cuba in 1961–1962, and the Dominican Republic in 1965. Our actions signaled to the Soviet Union that we were willing "to live in a world system of ideologically orthodox, superpower dominated regional blocs or ghettos." (p. 53). The full extension of this principle is the Johnson Doctrine, in which the United States explicitly stated that we had a right to intervene to prevent the establishment of a communist government in the Western Hemisphere. The similarity in the Brezhnev and Johnson verbal strategies the authors call the "echo phenomenon." The policies represent hypocritical mirror images of justification and condemnation depending upon whether a superpower is intervening or not.

Franck and Weisband blend their analysis of superpower interventions into a theoretical framework of systems theory, communications theory and bargaining theory. Arguing that the confluence of the Brezhnev and Johnson Doctrines is a bad bargain, the authors contend that the costs outweigh the benefits, not only in terms of moral altruism, but also in terms of self-interest.

They persuasively argue that words play a key role in forming mutual expectations, and insofar as they do, the verbal strategy of a nation should correspond to foreign-policy intentions. The international system, the authors contend, is characterized by behavioral regularities, and those regularities are a function of reciprocity. Insofar as a nation, especially a superpower, initiates a new pattern of interaction, it not only begins to transform the system but invites reciprocal behavior *even if it was not intended*. Verbal strategies which are applied consistently, and are accorded to others, are reciprocal and contribute to system stability.

Deterrence is an example of a verbal strategy of hostility, the authors argue, and is a relationship which depends upon predictable expectations. Insofar as a nation fails to deter itself from the actions it seeks to prevent in others, it is using a double standard and weakening its own credibility. Thus verbal strategies consistent with action, or vice versa, contribute to system stability through credible deterrence.

Franck and Weisband conclude by arguing that the Nixon Doctrine represents a verbal strategy of cooperation. President Nixon, they suggest, has drawn back from the extreme position of the Johnson Doctrine and is taking a more tolerant attitude toward the domestic political arrangements of other nations. Franck and Weisband suggest:

> We are at the crossroads. If they and we so choose, the Soviet Union and the United States can move from the dual ghetto pattern of subsystemic interaction to a new norm that safeguards the superpowers' justified, essential strategic interests, while yet permitting much greater expression of the national individuality of smaller states within the superpowers' region (p. 169).

There is much of value in this book not the least of which is the emphasis on long-term stability in the international system based not upon *realpolitik* but upon the principle of reciprocity. The authors effectively argue that an emphasis on actions and not on words is a sterile approach to long-run crisis management and international stability. In addition the implication that initiating cooperative moves may be based on principles of long-run national interests, and not simply misguided idealism, is a valuable one for policy makers in Washington and Moscow.

Another valuable aspect of this book is that it represents a synthesis of various modes of analysis and theoretical perspectives. The combination of case studies with the theoretical insight of systems theory, bargaining theory, and communications theory is a contribution that enhances both modes of analysis, relieving the case studies of their idiosyncratic quality while providing theory with empirical content. Franck and Weisband go beyond this, however, and move into the realm of policy prescription. The authors suggest general norms for the formation of verbal strategy, as well as offering specific policies for initiating spirals of cooperative behavior. Such policy suggestions are most welcome after years of inflated rhetoric in American foreign policy.

One weakness of the book is the tenuous link established between United States verbal strategy and its direct influence on Soviet verbal strategy. Another weakness is the suggestion that the Nixon Doctrine is one of cooperative verbal strategy. A few quotations (even if repeated on succeeding pages) do not a policy make. President Nixon's actions in foreign affairs undeniably have been bold and innovative, but the same man who is visiting China also invaded Cambodia, renewed the bombing of North Vietnam, and appointed Frank Shakespeare head of the USIA. It is difficult for this reviewer to imagine the President repudiating the Johnson Doctrine if another Dominican type situation arose. As the authors suggest, nobody suffers politically in the United States for overperceiving communist threats.

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Appearance and Reality in International Relations. By Grant Hugo. (New York: Columbia University Press, 1970. Pp. 207. $5.95.)

This is not a long book, and it is written in clear English. Still, it is not an easy book to read, primarily because of its concern with abstractions and their relationships. There is an occasional "nugget" of fact or anecdote or quotation, mostly intriguing or interesting. But the author on the whole attempts to stay with his own dry and self-prescribed theme.

His major proposition is stated on pp. 9–10: "that the quarrels of mankind are aggravated by misunderstanding" whose results "might . . . be mitigated if governments were to precede their decisions by a more methodical process of analysis" which would avoid the exacerbation which comes when "the participants are more concerned with motives than with consequences." Hugo argues that the only real difference between one decision and another is to be seen in the concrete results and that rational decision makers can and should be concerned with these predictable consequences.

Having laid down these points in an introduction, Hugo leads off with a hard-hitting chapter on "cant in foreign policy." On p. 29 he remarks: "It is the invocation of principles and the attribution of motives that identifies cant and not the merits of either." Earlier (p. 23) Hugo identified the "most characteristic feature of cant" as its sublimation of human suffering, its "abstraction of the argument from the details of death and destruction."

Turning then to an investigation of disputes he concludes (p. 55) that the real key to discrimination between disputes which should be applied is through prediction of terminal situations—which in turn rests (p. 65) on "the ability to assess the intentions of the other party . . . ." Hugo notes (p. 68) that intentions can be described in different ways, one of which "is by the attribution of motives"; but he dismisses this as wasteful, insisting that only the terminal situation counts. His illustration is the Cuban missile crisis, the argument over whether the missiles were "defensive," as compared to the terminal situation described by Kennedy as unacceptable: "a [Soviet] nuclear strike capability against the Western Hemisphere." In turn, Hugo argues that terminal situations which will probably be considered objectionable are those that "would entail a change in the established pattern of an international relationship," which produces "a threat of instability, and hence of crisis, proportional to the pace, extent, and unexpected character of the change" (p. 88).

Necessarily, Hugo then must examine "prediction" in Chapter 3. Perhaps his most important point here is that governments frequently do not continue doing in the future what they did in the past. A short Chapter 4 on abstractions, which looks at "reliability"—"an abstraction is useful when it describes a concrete situation . . . in terms that facilitate verifiable predictions"—is followed by a look at "prestige" and "influence" in Asian settings. These considerations take us through Chapter 6, with Hugo toward the end of this portion (p. 185) saying that prestige and influence were selected for this detailed look because each was a relatively neutral concept and could be employed by governments "of very different preconceptions" as a way of looking at "long-term considerations" (and predicting consequences?). They are not, he says, the only abstractions in whose name politicians urge the subordination of the present to the future. Other long-term considerations are, says Hugo, fairly useless in the contemplation of terminal situations. "Today men no longer plant oaks to provide their grandchildren with shade. . . . If we can predict, and plan for, an immediately advantageous terminal situation, we have already achieved much" (p. 187). Chapter 7, "end of argument," does not, unfortunately, sum up but rather wanders around making loosely connected philosophical observations. Hugo begins it by remarking (p. 188) that "the pursuit of abstractions has carried us far from our purpose (of rigor in) predicting terminal situations."

As this summary indicates, unless I have lost track of the argument in its later stages, the argument itself tends to lose much of its centrality. Yet some of the best analysis of actual disputes and situations occurs in those portions of the book most loosely connected to the main argument. Some of Hugo's phrases are masterful, for example, . . . "that sense of inevitability which is the essence of prestige" (p. 148). There are also other problems with the book. The chapter on "prediction" was written before the main text and, unfortunately, not thereafter altered. So it fits awkwardly. In it Hugo analyzes the Munich episode, concluding correctly that Chamberlain came away from the meeting content but, after Hitler's March 1939 absorption of the rest of Czechoslovakia, abruptly turned his policy around because "the occupation of Prague frightened both government and people out of their wits." In other words, the British were happy at first with the outcome of Munich and then very unhappy. How does this relate to Hugo's stress on terminal situations? When is a situation terminated, anyhow? This point gets no satisfactory analysis, yet it is fundamental to Hugo's main thesis. And are intentions to be considered preordained and uninfluenced by successive crises and their resolution? Yet Hugo says that divining intentions is of extreme importance.

On balance this book's attempt at a rigorous logical structure falls short of its goal. Nonetheless it is well worth reading, it is witty and frequently wise, and it is a significant contribution to the literature of international relations.

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The U.A.R. in Africa: Egypt's Policy under Nasser. By Tareq Y Ismael. (Evanston: Northwestern University Press, 1971. Pp. 258. $9.75.)

This review and analysis of nearly two decades of Egyptian policy toward Africa is characterized by an even-handed, dispassionate approach and by thorough research into local sources. The study does not concern itself with the making of foreign policy, which in Egypt's case is more difficult to research than most, but rather with its external manifestations, instruments, directions and encounters. Parts of the study are political history, for it would be difficult to analyze foreign policy out of time, context, and sequence. But other parts are more analytic, although using concepts organizationally rather than dynamically or relationally, and seeking to develop major characteristics of the late President Nasser's policy.

It is clear from the study that Egypt felt it was a model and had a mission in its relations with its southern neighbors. Its ancient civilization, its early sovereignty, and its growing industry made it annoyingly overeager to act as a big brother to lesser breeds without the experience, and as a result its successes were limited. (I have been assured by Egyptian diplomats that Nasser was more popular throughout Tunisia than Bourguiba, and by Black African diplomats that the UAR acts like a "bully" in the OAU caucus.) "Model" and "mission"—as America, Russia, and China are also learning—are too close to colonial notions to be attractive, leading to the dilemma that the very elements which give drive to a state's foreign policy are those which hinder its effectiveness.

Another characteristic that is well handled in Ismael's work is the clash between Arab and African interests, between Egypt's Arabness and its Africanness. The issue was often wrongly posed, by critics and defenders alike, as to whether Egypt had interests or identity with Africa, or whether the African and Arab world were in conflict. The problem has not been one of conflict between the two parties themselves but of conflict over the paramountcy of different issues, just as the problem is not whether Egypt is legitimately African on African questions but which side it identifies with when interests vary. Clearly, from this study, Egypt's African policy has been used to gain support for its Arab policy and not the reverse; Egypt wants to be involved in any decisions on African problems, but most Africans do not want to become entangled in Egypt's problems (p. 70).

Another characteristic is the incremental nature of Egyptian policy, its change and development in different stages of the Nasserite period, and its search to find and redefine itself in terms of Egypt's growing Arabness, revolutionary imagery, and historic interests. National interest is well explained in regard to Sudan particularly (pp. 164–5), although the important larger question of objective, determinable interests is not confronted specifically.

The book begins with a brief review of past Egyptian relations with its continent, an outline of the phases of Nasser's African policy, and a background review of Egypt's Cold War relations. It then examines the African policy in terms of principles (anti-imperialism, African-ism, leadership), objectives (minimum, intermediate, maximum, economic; or Nile, African self-determination, irredentism, and food, markets and raw materials), and instruments (diplomacy, culture, religion, propaganda, aid). Two case studies bring to light some new details on relations with Sudan and summarize UAR involvement in the prolonged Congo crisis. A conclusion predicts continuity after Nasser, which seems safe; it is more controversial in suggesting that "Egypt's best hope is to become the 'Japan of Africa'; that is, to promote substantial economic relations without political involvement" (p. 232).

Ismael's study is tighter, closer to its data, less anecdotal, more balanced, but less wide-ranging in its subject matter than its most direct competitor, Jacques Baulin's *Arab Role in Africa* (Penguin, 1962), and it is a good supplement to Charles Cremeans' *The Arabs and the World* (Praeger, 1963), although both are now out of print. It is a bit surprising that the author does not make reference to these works, if only to confirm or debate their findings; indeed, there is scarcely more than passing reference to any similar or conceptual work on foreign policy, which could have strengthened the analysis. Some other points could be developed to improve the message: there is no explanation of the Sudanese border arrangement as a background to the 1958 border dispute (p. 184), no comparison of UAR aid terms and programs with others' to show any reasons for a country's taking Egyptian aid (pp. 157–59), and no explicit juxtaposition of the two foremost concepts in foreign policy analysis: interest and power. Yet Egypt's policy shows that there is a relationship between ends and means, and that it is not simply a one-way learning process: interest without commensurate power may diminish, but it may also motivate greater attention to building a power base. This book will help scholars continue to study a dynamic policy.

I. WILLIAM ZARTMAN

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Communist China and Latin America, 1959–67. By Cecil Johnson. (New York: Columbia University Press, 1970. Pp. 344. $9.95.)

Cecil Johnson has written a thorough and intensive survey of China's relations with Latin America through 1967; despite the impressive size of this volume, however, Latin America is still not only that area of the Third World most distant geographically from China but the one with which China has to date (August 1971) found it most difficult to develop meaningful relations.

If Latin America is compared with Asia and Africa, the other two major developing continental areas, we find that China has had far more extensive relations with the Communist parties of Asia as well as with many Asian governments, be they Communist, neutral, or even anti-Communist; although Africa has few Communist parties with which China can deal, many of its governments have had commitments to the kind of revolutionary change the Chinese admire. Also, China has even had good relations with several nonrevolutionary governments in Africa. In contrast, Latin American governments have been slow to accept China: Only Cuba and Chile have recognized it, and even Bolivia and Peru have not yet recognized the Chinese People's Republic. No guerrilla movements have materialized since the Cuban victory in 1959. And since the early 1960s, only splinter Communist parties—not regular, pro-Moscow ones—have had relations with China.

China's lack of success in Latin America notwithstanding, Cecil Johnson's book is an excellent *tour d'horizon* of China's relations with that area; indeed, in certain respects he provides us with more than our money's worth. For example, his good, long chapter, "The Chinese Concept of People's War" really has something of the flavor of overkill in it: the more one reads it, the more one concludes how really *very* irrelevant the Chinese revolutionary experience and model may be for Latin America. As the Chinese and Vietnamese historical experiences with guerrilla warfare show, it is not easy to develop a sufficiently strong politico-military capability; Regis Debray's romanticization of the Cuban experience may only have served to delude revolutionaries, be they in Latin America or elsewhere, about the complex nature of their task—as Che Guevara's mission in Bolivia showed.

Indeed, while the Chinese, as Johnson correctly notes, carried no brief for that mission, its failure may have had an impact on the Chinese similar to that on the Cubans: a gradual but unmistakable decline in expectations that revolutionary change in Latin America could be accomplished by armed struggle. Hence, most pro-Chinese splinter parties in Latin America have merely continued to talk about revolution—but have not acted. Furthermore, this reviewer's survey of *Peking Review* for the past several years shows a steady decline in Chinese attention to Latin America as a whole, particularly vis-à-vis the splinter parties.

What does this all add up to? It tells us, I believe, that China's interest in the support of revolutionary movements generally and in Latin America specifically is declining, and the

Chinese may be reluctantly and painfully concluding that for the time being, guerrilla warfare does not have much of a future in Latin America. An article in *Peking Review* of June 25, 1971, "Latin America: Unite to Oppose Despotic Powers" stressed China's support for various Latin American *nations* in their political and economic conflicts with the U.S.; but no references to pro-Chinese *splinter parties' activities* appeared in that article, or in any other article dealing with Latin America during the first six months of 1971. Instead, Chinese articles reverted very much to themes of the mid- and late-1950s: commonalities of interest between China and Latin American nations against their "common enemy," the U.S. China was speaking like a Great Power seeking an involvement, however, limited, in inter-nation differences in Latin America, not in publicly associating itself with apparently hopeless parlor debates among small groups of Latin American self-styled revolutionaries.

The shift in China's policy concerns toward Latin America in the late 1960s and early 1970s is, of course, not isolated from general trends in Chinese foreign policy. The revolutionary aspects of Chinese foreign policy, perhaps more by necessity than desire, have had to be subordinated to the opportunities and problems which China faces in dealing with powerful rivals: the USSR, the U.S., and Japan. Confronted by such adversaries, limited by the resources it can spend on Chinese foreign policy, China since the Cultural Revolution—with the exception of Indochina—has reduced its material commitment to supporting revolutionary change abroad.

Judging by recent events, one cannot fault the Chinese policy makers for this decision. As far as Latin America is concerned, undoubtedly before this review is published, barring major internal changes, Peru and Bolivia will have voted unequivocally for the Chinese People's Republic's entry into the U.N. and will have established diplomatic relations with Peking. By entering into such arrangements, China will be tacitly signaling to other Latin American nations that it is willing to deal with those governments which come to power through elections (as occurred in Chile) or radical-nationalist military coups d'état (e.g., Peru and Bolivia); even other types need not be excluded. The day of attachment to the Chinese model as the *only* way for Latin Americans to attain fundamental change in their respective countries may be over at least for the next several years.

With this as background I was particularly pleased by Professor Johnson's excellent chapter comparing similar and dissimilar aspects of the Chinese and Cuban revolutionary models. This is an important and well thought out chapter, although one may want to reassess the Debray role at some future date. The book is not without some, perhaps unavoidable, shortcomings. There is no intimation that Professor Johns actually traveled to much, if any, of Latin America in order to get a better feel for the area as well as, in some cases at least, supplement translated sources with a wider variety of Spanish or Portuguese language sources than are available in the U.S. Additionally, Marco Antonio Yong Sosa's nearly ten-year guerrilla movement in Guatemala merits more treatment than Johnson gives it. But the fault may not be his: none of us interested in Chinese relations with Latin America have, unfortunately, had the time or resources to wade through the Russian, Cuban, Chinese, Guatemalan, and other Latin American sources necessary to have written authoritatively about the one guerrilla movement in Latin America in the past decade which *may* have had the potential to expand into a full-fledged guerrilla war, but, for reasons we must leave to other scholars to determine, failed to do so.

The literature on China's relations with different parts of the Third World is still remarkably thin. Cecil Johnson's book must be viewed within that *genre*. As such, it should be considered a very substantial effort to summarize what the author considered to be the major aspects of China's policies toward Latin America from 1949–67. While the author would undoubtedly be among the first to disclaim that his book is the definitive work on the subject, that his is the first full-length book to deal with Chinese relations with Latin America since 1949 should in no way demean its quality. If Johnson has not answered all the questions, he has at least charted the path for others—teachers and students—to ask more questions and seek their own answers. Thus, the book is a distinct contribution to the general literature on Chinese foreign policy, particularly with regard to the developing nations, and should consequently enhance our understanding of that policy—both before and after 1967.

DANIEL TRETIAK

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Britain and West Germany: Changing Societies and the Future of Foreign Policy. Edited by Karl Kaiser and Roger Morgan. (London and New York: Oxford University Press, 1971. Pp. 294. $13.00.)

The papers in this volume were originally presented at a Conference held in London under the auspices of the Royal Institute of International Affairs (Chatham House) in 1969; most of them have clearly been brought more up to date since then though not to the point of Britain's final and successful bid to become a signatory of the Treaty of Rome. As in so many books of this kind, the general theme is not always sufficiently central; the contributions are very uneven in quality; and the impression made upon the reader is not altogether clear. In the present case there is an added difficulty in that the symposium attempts two different tasks—to examine the foreign policies of Great Britain and West Germany in general and toward each other in particular, and also to use the two countries as examples through which to illustrate general concepts respecting the influence of social and institutional factors upon foreign policy.

While some authors can do this kind of thing rather well, and in a way which illuminates and explains the policies themselves, others are bemused by the so-called scientific approach to the study of foreign policy or international relations, and pile unilluminating abstraction upon unilluminating abstraction. Most of the concepts used in this area are of course of American origin; and it is worth noting that it is the German rather than the British contributors who make most use of them. It confirms the view that when French and German "Gaullists" talk of Britain as America's "Trojan Horse" in Europe, they are talking nonsense; the intellectual impact of the United States has been much greater on the Continent than in postwar Britain, where strong resistance to this impact is still put up by British scholars in the name of the traditional historically-oriented approach to such studies. One must of course take into account the fact that the papers by the German contributors have been translated from German; and therefore the opaque impression many of them create may be because what we are getting are American ideas translated into German and then back again into English.

Some chapters therefore make severe demands upon the readers who seek to get at the author's intention; such obscurity also enables authors to hint at practical consequences of their theories without needing to argue the case. Thus when Mr. Wolf-Dieter Narr states (p. 109) that "only a democratic political system permits a democratic foreign policy," one might dismiss this as mere verbiage since the meaning of "democratic" in relation to foreign policy is nowhere spelled out. But if one follows certain clues like the phrase "so-called student unrest" one can find out in the end that what we are getting is a dressed-up version of the "New Left" view of German foreign policy in which "anti-Communism" becomes responsible for the division of Germany and Europe, rather than the ruthless use of force by the Soviet Union. We are left to guess what the author means by a "real peace policy." Mr. Philip Abrams who writes the corresponding chapter on the British side is by contrast very illuminating on the social context of British foreign policy, and on the way in which for understandable reasons the young Briton is brought up with the view of the Germans as the natural enemy, as he finds him in the history books and young people's fiction alike; but even Mr. Abrams succumbs to the sentimentality about youth so common among Western intellectuals in his view that "youth culture" may be an important new factor in the future orientation of British policy.

The heart of the Anglo-German problem has been that Britain was forced in the immediate postwar years to swallow its hostility and resentment toward Germany and to cooperate in its economic rebuilding and eventual political and military rehabilitation as an ally. For this reason there must always be a danger, as is pointed out by Mr. D. C. Watt in the most important and useful of the essays, that the British desire for economies on defense, unsatisfied with the withdrawal from "east of Suez" will one day demand that British troops be withdrawn from Germany, and that this will cause a major upset extending beyond the confines of the Anglo-German relationship. For as Mr. Watt also shows, there is an inherent limitation in keeping to Anglo-German relations themselves as one's topic; little can usefully be said about them in recent times that does not also bring in the two countries' relations with France and the United States.

As Mr. Michael Donelan points out, there is a certain convergence between the Britain and German positions, if the world be considered as a whole. Britain is retreating into Europe just as Germany is beginning to look outside it; where the Third World is concerned, they now have a certain natural identity of interest and approach. On the other hand there is nothing in the British situation which corresponds to the equivocation forced upon German politicians by Dr. Adenauer. He insisted on the view that alignment with the West and reunification were complementary and not alternative objectives. Mr. Arnulf Baring makes the very well taken point that the weakness of the institutions of the Federal Republic in respect of foreign policy is shown by the absence of any serious challenge to this argument of Adenauer's. It was at least a question that deserved debate. But Britain also is still highly elitist on major issues of foreign policy. Britain opted for Europe because of a change of heart in Whitehall, not in the country at large, and the later political difficulties of getting into Europe are not unrelated to that fact. Books of this kind risk being overtaken by events. When he wrote his paper, Mr. Watt took seriously the NAFTA idea as an alternative way forward for Britain. No one now would regard such an arrangement as within the bounds of possibility.

MAX BELOFF

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Military Power and Potential. By Klaus Knorr. (Lexington, Massachusetts: D. C. Heath and Company, 1970. Pp. viii, 150. $10.00.)

No concept in the study of international relations is so ubiquitous as power, and no activity so dear to the heart of scholars as its analysis and measurement. Ever since the publication of his *War Potential of Nations* in 1955, Professor Knorr has been prominently identified with these efforts, and this book represents a major revision of his earlier work. The stated purpose of these changes is twofold: to incorporate the latest advances in the social sciences, and to take cognizance of the changes in the composition and uses of military power that have occurred in the intervening decade and a half. It appears, however, that he has not entirely succeeded, as this considerably shorter treatment (150 pages compared with more than 300 in the earlier book) poses far more questions than it answers.

The author begins with an extensive treatment of the meaning, uses and limitations of the concept of military power. In many ways, these first two chapters are the most valuable part of the book, providing a concise yet sophisticated summary of a complex and controversial subject. Traversing familiar ground, he distinguishes among *actualized* power (the visible change in behavior produced by military coercion), *putative* power (the possession of the coercive instruments which may be used to produce such change), and power *potential* (the possession of those human and material resources needed to acquire such instrumentalities). This discussion highlights the perennial dilemma confronting those who would make power an indispensable concept. On the one hand, by focusing on power potential (or even putative power) one pays a high price in theoretical and practical relevance, since only when we use the term in the sense of actualized influence may we assert a direct connection between national power and international outcomes. On the other hand, to use the concept in this way makes the task of *measuring* national power cruelly hard; it is far easier to compare nations' human and material resource bases, or even their relative military strengths, than to devise means of evaluating the *impact* of these capabilities on outcomes in the international system.

In common with most works on the subject, this book focuses on putative power as the most reasonable compromise between these competing demands. But however adequate this solution may have been in the nineteenth and first half of the twentieth centuries, developments in the post-World War II international system have rendered it less and less workable by complicating the already problematic relationships between power potential and military strength, and between military strength and actual influence. It is to the author's credit that he freely admits and discusses these drawbacks.

To begin with, the development of nuclear explosives as a central component of some nations' military strength has made it much more difficult to translate military might into actual influence. Since they are too dangerous to use except in the ultimate crisis, they contribute to a nation's influence only via the *threat* (explicit or implicit) of their use; yet, this very reluctance to use nuclear weapons renders such a threat very difficult to evaluate. These weapons also attenuate the connection between military strength and power *potential* by ruling out the possibility of mobilizing civilian resources in all-out strategic conflicts.

The rapid proliferation and growing importance of so-called "unconventional" conflict poses a second problem for the analysis of military power. Because of their distinctive tactics and military organization these conflicts give the lie to conventional evaluations of military strength. Moreover, because the political objectives of the participants differ from those of conventional war, there is often little correspondence between military strength and influence over outcomes. Finally, in many of these conflicts one of the protagonists is not a nation-state at all but a subnational or transnational actor. What does it mean to speak of the military strength—let alone the power potential—of nonnational actors? Clearly, any extension of these concepts to such cases must result in some major changes in their operational meaning.

Unfortunately, having raised these points, Professor Knorr does not see fit to incorporate them to any extent in his subsequent detailed discussion of the components of military strength. For one thing, his tendency throughout the analysis is to assume that the possession of nuclear weapons, their associated delivery and detection systems, and the technostructure for their manufacture and improvement is the *sine qua non* of contemporary military power. Since the real contribution to national influence made by nuclear weapons is so difficult to determine, and since all post-WWII military engagements have in fact been fought with conventional armament, it is hard to understand why more attention has not been paid to the trade-offs involved in emphasizing nuclear as opposed to conventional military capacity. It is still more difficult to comprehend why more stress has not been given to the problems of evaluating strength in unconventional combat situations. Given that such wars represent an increasing proportion of the total number of violent conflicts, and considering the havoc they wreak on power rankings computed on a "conventional" basis, it is scarcely sufficient to consider them as "special" situations apart from (and by implication not central to) the analysis of power potential.

Moreover, the heavy emphasis on the most technically sophisticated weapons means that these three final chapters constitute less an analysis of military power in general than an examination of the foundations of the American military industrial machine. Under virtually every heading, the greatest amount of material is directed toward describing and accounting for supposed American superiority. The results, on occasion, are almost ludicrous; in a passage he quotes to describe the attitude towards research and development in the U.S., it is asserted that "all the driving forces of American society have been marked with a scientific orientation —the major goals set by responsible leaders are not longer challenged—the scientific and technological effort is therefore deployed in almost total consensus" (p. 88). While thus inflating American capabilities, the author depreciates the capacities of less developed nations. They may indeed be capable of producing only "*Local* military power," but experience in Indochina and Korea seems to indicate that they can often do so on a scale sufficient to check even a superpower.

All this should not be considered as detracting from the book's qualities as a sound, traditional treatment of the bases of military power. But readers who are looking for a discussion of this problem in the light of contemporary warfare would be well advised to look elsewhere.

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The International Regulation of Frontier Disputes. Edited by Evan Luard. (New York: Praeger, 1970. Pp. 247.)

The international system, like its municipal counterparts, has developed procedures and techniques for dispute resolution. These include traditional political or diplomatic procedures such as inquiry or fact-finding, conciliation, negotiation, and mediation. Other procedures are of a juridical nature and include arbitration and judicial settlement. Within this century international and regional organizations have added another dimension to techniques of peaceful settlement. Evan Luard, Member of Parliament and supernumerary fellow of St. Antony's College, Oxford, has edited an admirable volume which examines these techniques of dispute resolution within the context of frontier conflict.

Luard notes that historically frontier disputes have been the single most frequent cause of war between states. While that is not true today, at least in the sense of revisionist powers seeking to alter frontiers not yet firmly established, our own conflicts, with their ideological and postcolonial mix, still often have contained elements of frontier conflict. Such conflict before the advent of nuclear weapons often led to total conquest. Since World War II, however, frontier disputes, like all other forms of conflict in the nuclear age, have been limited in scope and in objectives.

Another difference noted by Luard is that since 1945 all such disputes have occurred outside Europe, whereas the major conflicts from World War I to 1945 had taken place within the continent. This distinction reflects at the same time the growing stability of European boundaries and the emergence of new nations outside Europe often willing to dispute the legitimacy of frontiers established during a colonial past.

Stanley Windlass, writing on the League of Nations and territorial disputes, notes that most of the conflicts handled by the League were border or territorial disputes stemming from the disintegration of the Austro-Hungarian, Turkish and Russian empires. Franco-British solidarity allowed the League to enjoy impressive success in resolving most of the disputes which arose in the 1920s. This proved insufficient to deal with the aggressive acts of other great powers in the 1930s. Windlass concludes that "in no case . . . is it possible to say that the final outcome of the dispute was radically different because of the presence of the League" (p. 83). I disagree. The dispute of Albania against Yugoslavia and Greece in 1921 might well have resulted in substantial territorial losses, if not the disappearance of Albania, but for the actions of the Council of the League and its commission of inquiry which helped to establish the Albanian government and to settle that state's frontiers. Again, the Demir-Kapu frontier dispute between Greece and Bulgaria in 1925 might well have resulted in another Balkan war but for the forceful demands of Aristide Briand, President of the Council of the League. Greece, prepared to invade Bulgarian territory, pulled back after reception of Briand's telegram demanding that neither side resort to war. A commission of inquiry sponsored by the Council was later instrumental in settling the dispute. The eventual failure of the League has made it all too possible to forget its impressive successes in dealing with disputes of lesser magnitude than Manchuria or Spain, but still quite sufficient to have resulted in war in the absence of effective regimes of settlement.

Alan James, of the London School of Economics, analyzes the relative lack of success enjoyed by the United Nations as contrasted with the League in dealing with frontier disputes. First, the disputes dealt with by the League were largely pure frontier disputes, as noted, stemming from the disintegration of imperial systems. These disputes did not possess the ideological context, nor were they enmeshed in anything similar to the present bipolar Cold War rivalry in which every territorial claim is taken to represent a geopolitical probe or thrust by the other side. The relative success of the U.N. in bringing hostilities to a halt, as opposed to resolving the underlying dispute, does not reflect any mechanical deficiency within the U.N. system but rather stems from these basic differences between the international systems before and after World War II. The United Nations, to be able to act in its corporate capacity in a sense apart from its constituent members, must function largely under Chapter VII powers. This in turn demands several prerequisites, including a certain measure of big power unity. Most peaceful settlement machinery falls under Chapter VI and here the United Nations is not really an entity to be judged apart from its member states.

Our world, jaded and made cynical by ideological disputes between the superpowers, no longer looks with hope toward the possibility of impartial judicial or diplomatic institutions which can remain free from bloc politics. Communist dogma rejects the very possibility of such a superstructure free from the ideological consequences of its economic underpinnings. Fact-finding, good offices and mediation are relatively free from these factors in that such techniques do not interpose a supposedly impartial institution between the parties but rather in large part seek merely to bring them together. Disputants are more likely to agree to these more informal means of dispute resolution than to arbitration or judicial settlement because in so doing they retain the right to reject the suggestions of the mediator.

Arbitration and its cousin, judicial settlement, have both been ineffective in settling frontier disputes. Henry Darwin, Legal Counsellor in the United Kingdom Permanent Mission to the United Nations, in discussing the history of the Permanent Court and its successor, the International Court of Justice, concludes (with a statement possessed of an understandable touch of Robert Ardrey) that:

> . . . the main impression which results from a study of the cases . . . is how few the cases are and how small the areas of territory involved. . . . It is hard to escape the conclusion that this reluctance stems from a strong political sentiment and even perhaps a psychological instinct about national territory, whether actual or potential (p. 218).

Luard proposes several conclusions and recommendations of uneven quality. He observes that European boundaries are now more stable than ever before, and that pressures from antagonists are now directed more toward effecting internal change within a state rather than a formal revision of boundaries. He correctly concludes that it is not the absence of procedures and mechanisms which has prevented the peaceful resolution of disputes but rather the unwillingness of states to use existing machinery. Then, somewhat inconsistently, he proposes that the U.N. create an institution modeled on the League's Council of Ambassadors which helped determine European boundaries following the Treaty of Versailles. In that earlier time sufficient big-power unity existed to enforce boundary decisions by such a body. Today no such situation exists. An institution of this sort would be born only to be embalmed alongside the Permanent Court of Arbitration and the Panel of Arbitration and Conciliation.

Successful institutions of judicial settlement (and arbitration is actually a subcategory of this process) demand as a prerequisite a greater degree of community than actually exists in the international system. When compared with the communities which comprise the more established nation states one is speaking euphemistically to speak of the "international community." Given this condition the world will continue to rely most heavily upon the political or diplomatic processes of dispute resolution. These processes allow separate communities to negotiate solutions to disputes which divide them. Techniques of judicial settlement demand the placement of more trust than presently exists within institutions which rightly or wrongly are seen as being alien to the cultures of the separate communities. For the immediate future, at least, the major disputes which divide nations will be resolved either by war or by political and diplomatic processes.

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The German Rearmament Question: American Diplomacy and European Defense after World War II. By Robert McGeehan. (Urbana: University of Illinois Press, 1971. Pp. 280. $10.00.)

The immediate impact and the long-range consequences of the American decision to seek the rearmament of Germany, a few years after the end of World War II, can hardly be exaggerated. West German rearmament helped bring about the division of Germany and Europe; it became the central element of Washington's containment policy; it significantly affected the balance of power and influence within the Western alliance; it contributed to the search for integrative structures for the Western alliance and for Western Europe (economic as well as military) in order to control a renascent Germany; and it provided the Adenauer government with the leverage to regain sovereignty for the new West German state. The decision to rearm Germany was one of the major events of the postwar world.

This important subject has received relatively little scholarly attention in this country. In Germany, there are, among lesser works, Gerhard Wettig's monumental and authoritative *Entmilitarisierung und Wiederbewaffnung in Deutschland, 1943–1955* (Munich: Oldenbourg, 1967), which is still the most meticulous and most carefully researched account of the issue; and the recent monograph by Klaus von Schubert, *Wiederbewaffnung und Westintegration* (Stuttgart: Deutsche Verlagsanstalt, 1970), which concentrates on the connection between rearmament and integrative structures of the Western alliance. Professor McGeehan's book is thus a welcome contribution to the sparse literature in this field in the English language.

McGeehan's study treats the five-year period between 1950, when the issue of German rearmament emerged under the impact of the Korean War, and 1955, when Germany became a member of NATO. The study

> concerns itself in particular with the evolution of the U.S. demand for a German military contribution, from a simple call for more troops into an elaborate scenario for the future political unification of Western Europe. Its concluding thesis is that the German rearmament question, in leading to the European Defense Community (EDC) project, brought political results far more important than the military frustrations which attended the delay in the Federal Republic's remilitarization. Despite eventual French abortive action on EDC, its long gestation period was vital; it granted sufficient time for the adjustment of attitudes in Washington, Paris and Bonn which in the end made it possible for Germany to be rearmed and admitted to NATO on mutually acceptable terms. Without the intervening period of the American diplomatic pursuit of German rearmament and European political unification through EDC, the settlements of later years would not have been achieved as rapidly or perhaps as auspiciously (pp. xii–xiii).

The book's primary emphasis is on the American role in bringing about German rearmament; but McGeehan also deals with the intricate negotiations among the Western allies that preceded Germany's accession to NATO, tracing the tortuous path from the initial American decision to rearm Germany, made under the impact of the Korean War, to the French counterprosposal of the Pleven Plan and the demise of the European Defense Community, which, in turn, led to the admission of West Germany as a full and equal partner in the Western alliance. Most of the narrative covers familiar ground, and there is little that is new either in terms of source material or interpretation. But McGeehan tells his story effectively, and the material is organized in a way which allows the reader to follow a fairly complicated and many-sided issue with relative ease.

Of particular interest is McGeehan's demonstration of how the initial American interest in rearming Germany began to have wider and somewhat different ramifications than expected, making it imperative for the analyst to distinguish between the goals of 1950 and the accomplishments of 1955:

> On the record, American goals as announced by Washington in 1950 and for a considerable time thereafter were not achieved with respect to a German defense contribution. Yet in retrospect U.S. policy *was* successful; the Soviet Union was deterred, Europe recovered from economic weakness and political infirmity, while at least beginning a military basis for security; and the unknown factor of the instability which would automatically have been introduced by the sudden reappearance of German military power did not materialize (p. 252, emphasis in original).

In short, by 1955, Washington's policy to rearm Germany had turned out to be a success not because it enhanced NATO's conventional troop strength with a contribution of twelve German divisions—that was not accomplished until a decade later—but because it became the linchpin which held together the political, economic, and psychological elements of American containment policy in Europe. Perhaps this explains why the initial decision to rearm Germany proved to be remarkably immune to reconsideration or even extensive modification—this despite the fact that there was a change of administrations in Washington as well as a change in the military-strategic and political context in which negotiations over German rearmament took place, either of which might have occasioned a reappraisal of the cogency of the 1950 decision.

One reason for the continuing (in fact, growing) importance of a German military contribution in the middle 1950s was the changing nuclear-strategic relationship between the Soviet Union and the United States—a dimension which McGeehan tends to slight in terms of its implications for Washington's strategic doctrine and the role assigned in it for German contingents. When the Eisenhower Administration felt compelled to prop up its sagging posture of massive retaliation with the decision to fortify NATO with tactical nuclear weapons, German conventional forces were needed even more than before. The twelve divisions that the Bonn government had agreed to provide were now assigned two functions: one was to carry the major burden in defending the central sector of NATO, the other was to provide a strong forward position of ground troops, which was required by the new strategy to force the enemy to attack in concentration so as to make tactical nuclear counterstrikes efficacious. In addition to being part and parcel of the Western ground forces, the planned German contingents had become an indispensable element in Washington's attempt to fashion a new nuclear strategy for Europe.

Unfortunately, McGeehan also fails to analyze the issue of West German rearmament in the wider political context of East-West relations in the early 1950s. There are occasional references to Soviet moves, such as the Soviet note of March 10, 1952, calling for a peace settlement for all Germany; but even in the concluding chapter of the book ("The Question in Perspective") McGeehan does not raise the fundamental issue of what alternatives might have been open to Washington, relegating to a footnote George Kennan's well-reasoned objection to the military-strategic thrust of Washington's dress himself to the central questions, McGeehan has not fully exploited the rich material containment policy. By his reluctance to ad- presented in the book, leaving room for yet another study which would question the inherent logic of enlisting a rearmed West Germany in the service of American foreign policy in the postwar period.

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The Berlin Crisis, 1958–1962. By Jack M. Schick. (Philadelphia: University of Pennsylvania Press, 1971. Pp. 266. $9.50.)

General de Gaulle once remarked that the Soviet Union had "artifically" created the Berlin problem. However created, it assumed a nagging reality marked by recurring crises which threatened an unwanted but unavoidable war. Not until December, 1971, when the two German states worked out accords governing access to the isolated city (pursuant to the Four Power Treaty of the preceding September), could the citizens of West Berlin anticipate a somewhat normal existence for the first time since the Second World War.

During the last quarter-century, the Berlin question lay at the center of the East-West confrontation, politically, geographically, and psychologically. In this account of the 1958–1962 crisis, Jack M. Schick delineates the tortuous course of the most prolonged and dangerous Berlin drama of the postwar period, and illustrates the manifold considerations which made the German arena of such special concern to the Soviet leadership, to United States policy makers, and to the world at large.

Schick notes that the long confrontation "began and ended with a missile crisis" (p. 233). Its earlier phase was marked by Moscow's desire to capitalize upon what seemed to be the shifting of the strategic balance toward the Soviet Union, in light of the Sputnik achievement and the anticipated missile gap in Russia's favor. Hoping to translate military developments into diplomatic advantages, Khrushchev launched the Berlin crisis in order to block definitively what seemed to be American plans to allow Bonn access to nuclear weapons, and to achieve recognition of the authority of the East German regime. Its final phase became a test of wills which ended only when Khrushchev became convinced during the Cuban missile crisis that the United States *would* use force to defend its vital interests.

The four years following the Soviet ultimatum of November 27, 1958, which demanded progress toward a Berlin settlement within six months, brought numerous diplomatic maneuvers, summit meetings, and other ploys in the high politics of superpower encounters under the pressure of mutual threats. The author relates in considerable detail how these events led policy makers to misread each other's signals and intentions; how situations developed for which the protagonists had no prepared responses; how soldiers and statesmen tested the reactions of their adversaries often without the assurance that the risk was worth the reward. Mikoyan's taunt at the time the Wall was constructed that "only a lunatic" would go to war over the Berlin question (p. 178), and Robert Kennedy's public statement that the United States would use nuclear weapons in its defense, suggest that neither side was immune to a touch of madness, real or propagandistic.

The Berlin Crisis does not advance a reappraisal of either the origins or the principal events of the cold war, nor does the author purport to offer a theoretical addition to the literature of crisis diplomacy or conflict resolution. As a study of a particular subject, it is noteworthy not because of a unique interpretation or presentation of previously unavailable material, but for its thorough and coherent treatment in the traditional manner, its factual text embellished by numerous analytical judgments. It appears in the wake of a number of recent revisionist works, whose most salient thesis is that the Eastern menace was largely the product of the Western imagination. Schick's study frequently quotes the somewhat outrageous rhetorical outbursts of the opposing powers' spokesmen, and otherwise vividly reminds the reader of the tensions and apprehensions of these years. Reviewing the 1958–1962 period, one hesitates to invoke the admonition that things are not what they seem. Even a vigorous exterminator of cold war hobgoblins would be hard pressed to conclude that nothing was really at stake in Berlin, and therefore that it was preposterous that the powers should have come so close to war so many times over the question. The Berlin crisis demonstrates the axiom that the vital interests of the great powers are what their policy makers define them to be, sometimes apart from patently rational factors.

It is convenient, and in many ways correct, to proffer Berlin as a catch-all symbol of cold war Europe, the embodiment of the interplay of Soviet ambitions and Western resolve. Since the city's survival precluded an objective test of whether its symbolic importance was exaggerated, the tendency remains to portray the West Berlin story as a microcosm of the global struggle with communism. It may be true that the sensitivity of the Berlin issue was a physical reflection of the psychological vulnerability of all Germany. But why did Khrushchev emphasize so relentlessly that the Berlin situation was abnormal and intolerable? Why did Eisenhower, Dulles, and Kennedy perceive Berlin as the testing place of their willpower in the contest with totalitarian expansionism? Why did Adenauer and his ministers tie their prestige so intimately to absolute nonrecognition of the German Democratic Republic, when the Western allies had long since annointed the Bonn government as the sole legitimate voice of all Germans? Even after these partially unanswerable questions have been explored and explained in *The Berlin Crisis,* there remains an element of the absurd in this long flirtation near the brink of World War III.

While it is not fair to criticize Schick for not having written a different book, it is regrettable that this account of a crucial segment of postwar European history is not placed in the perspective of more recent developments such as the *Ostpolitik* of the Federal Republic, the intensified contacts between the two Germanys which have brought them to the edge of recognition, or current proposals for a conference on European security and cooperation. But these remarks are peripheral. *The Berlin Crisis* is an admirable work as it stands, a piece of sound and articulate scholarship, and an important companion to the best of the available literature on the subject.

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The United States and the Caribbean. Edited by Tad Szulc. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971. Pp. 212. $5.95.)

The title of this book is misleading, for the U.S. role in the Caribbean is touched upon only peripherally in eight essays focusing on recent political, social, and economic developments in the countries of the Caribbean. As might be expected, the essays vary widely in quality, focus, and level of analysis. The best are by Gordon K. Lewis, Anthony P. Maingot, Ben S. Stephansky, and Kalman Silvert.

Lewis, a British political scientist teaching at the University of Puerto Rico, persuasively and lucidly develops what emerges as the primary theme of the book, namely that "Caribbean politics are the politics of nationalism" (p. 16). He refers to the pervasive, emotional, and on recent occasion even violent rebellion of the Caribbean peoples against external dependence. Political independence is now for the most part a reality, but the metropolitan countries (Britain, the United States, France, the Netherlands) continue their cultural and, in conjunction with small local elites, their economic domination.

Caribbean nationalism, then, seeks to eliminate the last remnants of political dependency and, much more difficult, to gain economic autonomy. But perhaps its most important feature, Lewis implies, is that it embodies a search for distinctive national identities. In this sense the Cuban revolution remains very much a model for the area. Though its political and ideological components have not taken root elsewhere, Castroism's uncompromising economic, cultural, and psychological rejection of the past has obvious relevance and at least limited appeal to modernizing movements throughout the Caribbean.

Maingot, a young historian from Trinidad now teaching at Yale, develops these themes further. His focus is on the rapid crumbling of the traditional social structures of Caribbean societies and the rise of a mass-based Black Power movement. Maingot argues—as does Frank McDonald, though in a less balanced manner—that foreign domination of the Caribbean worked through and was facilitated by a highly inequitable local class structure, in which a tiny, mostly-white, wealthy minority was able to exploit or at least ignore the inert, impoverished, mostly-black masses. With the rapid rise of mobilization politics, however, of which the Black Power movement represents one form, the status and power of the local elites, like their foreign counterparts, is now under wide attack. Black power, then, already a growing political reality throughout the Caribbean, is at once a class, race, and psychological assault on the quasi-feudalist past and its present-day institutional remnants.

Stephansky reviews the dramatic economic development of Puerto Rico and examines the question of whether the Puerto Rican experience can serve as a model for the rest of the Caribbean. The answer is mixed—on the one hand the Puerto Ricans have certainly amassed a body of experience and expertise in development, but on the other their special relationship with the United States, which provided the basis for that development, is of course not transferable. Moreover, Stephansky makes clear that the perennial crisis of identity of the Puerto Ricans themselves is now most acute, with Munoz Marin's "commonwealth" solution losing its appeal to both the statehood and the independence movements. Hence, the form and extent of Puerto Rican leadership in the Caribbean must await a final resolution of this question.

Perhaps the richest and most provocative essay in the collection is by Kalman Silvert. Wide-ranging and original, the essay defies summary; suffice it to say that it is full of imaginative observations and insights not only about the Caribbean but about general processes of economic and political change. Silvert also briefly discusses the relationship of the Caribbean to the United States, emphasizing what he sees as the uncertainty and unpredictability of the U.S. role in the changing circumstances of the nuclear era. In his discussion, Silvert touches upon but does not develop an answer to what this reviewer considers to be the central question: Precisely why, if at all, is the Caribbean important to the United States? Nearly all Americans writing on U.S. policies in the Caribbean implicitly or explicitly *assume* the cruciality of the area to the United States, but hardly any see the need to *argue* the case. Tad Szulc's introduction to this collection is typical of this approach—he simply asserts with no further development that the Caribbean has "acquired a significance that cannot be ignored in serious policy considerations on the part of the United States." Why not? The economic importance of the area to the United States? Surely not, it has only a trivial impact on our economy. Perhaps the geographical propinquity of the Caribbean? Americans seemed to think that was quite important in the early 1960s. Recall the hysteria—"only ninety miles away"; "Cuba is a dagger pointing at the heart of America"; etc. But Cuba is now an all but dead issue in American politics. Can it be that public opinion and policy makers alike are belatedly, semiconsciously, and almost reluctantly coming to understand that traditional geopolitical conceptions of "security" are irrelevant in the nuclear era?

JEROME SLATER

SUNY at Buffalo

Trade and Investment Policies for the Seventies: New Challenges for the Atlantic Area and Japan. Edited by Pierre Uri. (New York and London: Praeger, 1971. Pp. xviii, $12.50.)

This collection of papers and discussions stems from a conference in Tokyo, sponsored jointly in March, 1971 by the Atlantic Institute of Paris and *Keidanren* (Federation of Economic Organizations) of Tokyo. It was published with unusual alacrity, appearing before the end of 1971. Even this, alas, was too slow; the collection is already dated by the "Nixon shock" of August 15, which it recognizes only in a politely antagonistic "Postface" by the editor (pp. 267–271). Indeed, a Bank of America representative, Mr. Constant Van Vlierden, had assured the Conference itself that "the Nixon government is fully committed to the principles of free trade, and . . . I see not the slightest reason to doubt that these principles will be adhered to (!)" (p. 229).

The "trade" papers are mostly the standard stuff fat cats tell each other across national boundaries. Everyone is for freer trade and investment—but also for "orderly markets" and against "disruption." Also, everyone wants to be sure everyone else understands his special reasons for postponing freedom of trade until Khrushchev's shrimp learns to whistle. Everyone likewise favors free competition, provided it is also "workable" and (in Japan particularly) also "guided." Everybody favors international arrangements to restrain large-company elephants' dances among small-company and small-country chickens, but eschews international antitrust legislation in favor of unenforceable gentlemen's agreements. Consumers, by the way, are apparently not among the parties to be protected by such agreements.

More interesting were the discussions dealing with the multinational corporation, which either will or will not dominate international trade and investment in the near future, and either will or will not repeal the law of comparative advantage in deciding its location problems. (It depends whose paper you read, Professor Jack Behrman's or Professor Lawrence Franko's; this reviewer is not qualified to decide between them.) There is agreement that American predominance among these companies is not the wave of the future, so that *Le Défi Américain* is a bit of a bogey. One misses, unless one reads between the lines of remarks by Japanese "respondents," any account of the reasons why the multinational corporation is hated and feared as "the corporate Ugly American" overseas. Also, an AFL-CIO spokesman could have made a splash in exposing the "runaway shop" and "job-exporting" aspects of multinational corporations' activities. Economists and economic Japanologists will also find uniquely informative Mr. Morihisa Emori's introduction to the world of "Japanese General Trading Companies" (pp. 111–124) (the least unknown of which are probably Mitsubishi Shoji and Mitsui Bussan), a specifically Japanese form of the multinational corporation.

M. Bronfenbrenner

Duke University

American and Soviet Aid. By Robert S. Walters. (Pittsburgh: University of Pittsburgh Press, 1971. Pp. 299. $9.95.)

American and Soviet Aid was not intended to be an historical work. Events have made it that. Disenchantment with aid in both countries is changing the shape of their aid programs. The U.S. program, in particular, is under great stress; and the outcome in terms of administrative structure and program content is anybody's guess. For Dr. Walters, of the University of Pittsburgh, the pace and extent of change is unfortunate; he has given us a very thorough description of the American and Soviet aid programs—as they used to be.

His book emphasizes the motivations for and mechanics of the programs—how they operated at home and abroad, their bureaucratic structures, the magnitudes and forms of aid, the terms of aid. This is good accurate stuff. Its pertinence is another matter. The key questions now are: What happened; what was the economic and political impact of these programs? Dr. Walters apparently agrees that these are the central issues, for it is on them that he focuses his conclusions. Why, then, the large quantity of data that cannot support those conclusions and the paucity of data that can? I find this aspect of the book puzzling.

To Dr. Walters, as to many students of foreign aid, the political impact is largely a function of the economic impact. He thus devotes a good portion of his argument to economic issues. I find that he accepts the AID economists' word for what they have achieved with fewer reservations than the evidence warrants. He notes, to cite one example, that PL480 food inputs were crucial in facilitating economic growth in recipient countries. This may be so. He should, at a minimum, however, have recognized the opposing view that, in some cases, the availability of PL480 food grains enables recipients to pursue inappropriate industrialization policies and to ignore agricultural development. This seems to have been the case in India.

The author deals extensively with the influence or leverage (over recipients' economic policies) that aid inputs afford. The magnitude of such influence is a matter of opinion. Dr. Walters comes down on the AID side here also. For example, he argues that India's growing emphasis on the private sector is attributable in large part to wise shepherding by the United States, but he offers no evidence to support this contention. It can be and has been posited that our efforts were marginal; and that the U.S supported a trend that would have existed without our involvement.

The author also appears satisfied that the U.S. employed its leverage wisely. In some cases, it undoubtedly did; in others, it clearly did not. AID and its predecessor agencies have learned the hard way. A number of aid recipients have been irritated with the poor results of policies foisted on them by U.S. officials. This irritation is not without political significance.

Finally, the author concludes his discussion of economic impact with the assertion that the United States' extensive experience in the aid business has equipped it better than the USSR or any other state to "deal with development through economic assistance." It would be nice to be able to agree.

Dr. Walters is less sanguine about the political impact of aid, but only slightly so. He believes that aid does have a short-run utility, that it can preempt another donor from gaining undue influence. He is also impressed—a bit uncritically—by AID's preoccupation with scoring political points on a day-by-day basis. Describing the aid situation in Afghanistan, he notes that the small U.S. program, which annoyed the Afghan government less than did the large Russian program, yielded "disproportionate benefits to the United States" (p. 244). What benefits? Did these benefits matter in terms of any serious or basic U.S. interest? Undoubtedly, as he points out, the donor with the largest presence in a country has the nose most tempting to tweak. Experience tells us that most recipients frequently are sufficiently annoyed to tweak, but smart enough to forego tweaking so hard that the tweakee will be driven away.

Dr. Walters, fortunately, goes beyond his unelaborated views that aid plus trade and investment somehow give political influence worth having. He concludes that the question of the long-run impact of aid on system maintenance or conversion is open. This is refreshing.

In sum, this work will be marginally helpful to diplomatic historians of the late 'fifties and the 'sixties. It will be quite helpful for students of foreign aid administration; and it may prove highly useful to future scholars who wish to compare the programs of the 'seventies with those of the 'sixties—if, in fact, bilateral programs survive.

EUGENE B. MIHALY
University of California, Berkeley

Japan's Postwar Defense Policy, 1947–1968. By Martin E. Weinstein. (New York and London: Columbia University Press, 1971. Pp. 160. $7.50.)

At a time when the subject of Japanese rearmament is hotly debated around the world, especially in Peking, Pyongyang, and Tokyo, this short book offers welcome reassurance that Japanese political leaders since 1947 charted their own defense policy along very moderate lines and resisted American efforts to rearm faster or more fully. Weinstein's doctoral thesis, based partly on his three years' Air Force duty in Japan and more on his subsequent year of thesis research, reflects the views of such Columbia University faculty as James W. Morley and probably would not convince many critics of the Tokyo conservative elite. Nevertheless, it is one of the few books in English on this important topic and should serve to stimulate others to amend or rebut the central thesis.

The book begins with a brief résumé of Japan's geographic and historical setting, stressing the traditional fears of Russia as the main strategic threat and of Korea as the main area of danger. The end of the Pacific War gave Russia valuable territorial concessions in northern islands never before claimed by Russia but held to this day, and in North Korea and Manchuria. The Japanese national public has voted Russia its most disliked foreign nation every month since the Jiji Press began its "liked and disliked nation" poll in 1957, with China and the two Koreas close behind. The book makes little reference to Japanese public opinion, however, or to the massive responsibility of the American Occupation for disarming Japan both physically and psychologically. No one can understand the postwar Japanese defense policy of limited rearmament without fuller consideration of these two determinants.

The Weinstein thesis is that Prime Minister Yoshida, who dominated Japanese politics from 1948 to 1954, set the defense policy for his successors. Yoshida felt that Japan would need an American security guarantee long after the peace treaty, even before the outbreak of the Korean war convinced Washington that it needed Japanese bases to contain China and Russia. The major portion of the book deals with the period from 1947 to 1960, especially the 1947–1952 years when the peace treaty and U.S. Security Treaty were debated by the two governments under less then equal conditions. One key argument is that Tokyo considered its offer of base rights an adequate contribution to the American side: *a quid pro quo* much less than the usual military alliance clause of mutual aid in event of attack on either party. The Japanese restrictions on U.S. bases since 1960 meant they could not be used for nuclear weapons or defense of other Asian countries (except the UN commitment to South Korea), and Under Secretary of State Alexis Johnson told the Senate in 1970 that bases in Japan were primarily designed for non-Japanese defense, so this *quid pro quo* is weak from the U.S. side.

Weinstein also cites the Japanese willingness to defend against a Communist takeover from within, which he seems to consider a reasonable threat in 1949-1952 against the view of most other writers. Domestic revolt in Japan has never been a serious threat, although the ultra-right in Japan constantly talks of a Peking/Moscow alliance with internal leftists. "Prime Ministers Yoshida, Hatoyama, Kishi, Ikeda, and Sato have made defense policy in the belief that the Soviet Union posed the principal if not the only external threat to Japan . . ." (p. 41). Again, the author has mistaken the anti-Communist rhetoric of Japanese leaders for their real intentions of expanding trade at any cost. Japan today is the major non-Communist trading partner of Russia and China, while the northern territorial issue is played down in relations with Moscow.

The intriguing thesis that Japan has been immune to U.S. rearmament pressures because Yoshida spurned Dulles's 1951 demand for a 350,000-man Japanese army ignores the substantial Japanese military budget increase of recent years when quality rather than manpower was the focus to appease the antirearmament public. This reviewer has interviewed dozens of Japanese conservative politicians and Foreign Ministry bureaucrats who attested to the pressure by Washington for bigger defense efforts and commitments. It is correct to say that Tokyo has not extended its defense to any neighboring area, nor would more than one-tenth of its public approve.

The book is less adequate in treating the 1952-1960 period and gives slight attention to the 1960-1968 era under the revised treaty which was automatically extended in 1970 on a year-to-year basis. In 1972, the treaty was extended to cover Okinawa when that prewar prefecture and its 900,000 people were returned to Japanese control. The big debate now is over the continued presence of any U.S. bases in either main-island Japan or Okinawa, whose population strongly opposes such bases. Weinstein does not really come to grips with these issues. He claims (p. 98) that the American promise not to introduce nuclear weapons into Japan has "certainly been honored," but some of my sources doubt that statement: and the Okinawan public turned against both U.S. and Japanese military presence in 1971 because of revulsion against the use of their island for the Vietnam war and related purposes.

In the summer of 1971 the reviewer talked with high American military officials who minimized the effect of Okinawan reversion on their bases despite the large number of facilities to be returned to Japanese control. He also found mounting opposition in Tokyo to the 4th Defense Plan budget at a time of recession and uncertainty due to the higher value of the yen in world markets, but also due to heavy loss of life in a plane accident caused by a Self-Defense Force pilot trainee. It may be true, as Weinstein claims on p. 122 that the public would accept a higher defense budget because there was no public riot against the 13 per cent of the national budget in the 1950s compared with 7 per cent in 1968, but he ignores the traumatic effect of the Vietnam war on Japanese opinion and the many other deterrents to heavy Japanese rearmament.

The book goes too far in exonerating the Japanese government of any complicity in American military strategy since the Korean war. Its main value is in giving the official Tokyo and Washington views of their defense arrangement, mainly to 1960. A far more complete account can be found in John Emmerson's new book, *Arms, Yen and Power: The Japanese Dilemma* (New York: Dunellen Press, 1971). The 135-page text is supplemented by texts of the 1951 and 1960 security treaties and an excellent bibliography. The high price of the book will deter most readers who can find it in libraries: a cheaper paperback edition would have enabled more people to buy this very useful and controversial study.

DOUGLAS H. MENDEL, JR.
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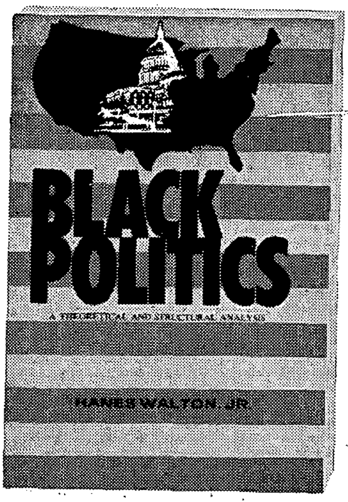

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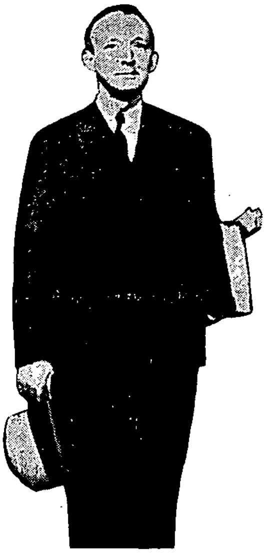

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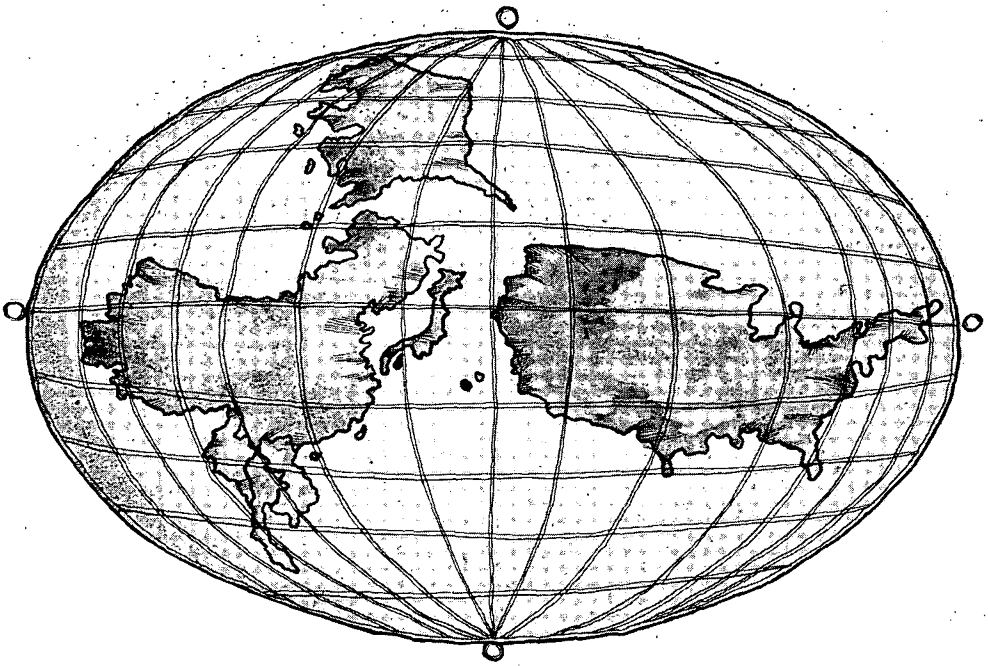

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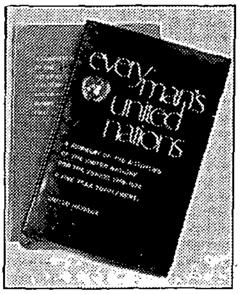

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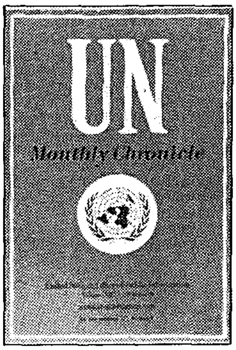

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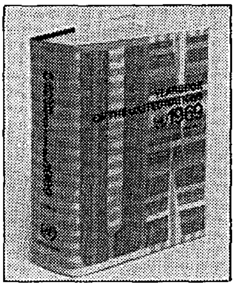

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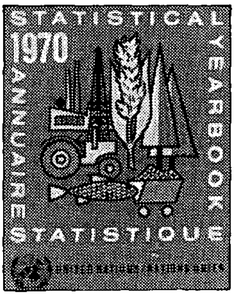

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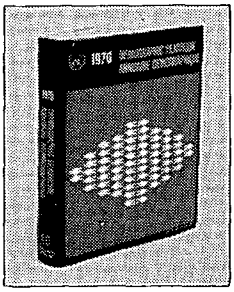

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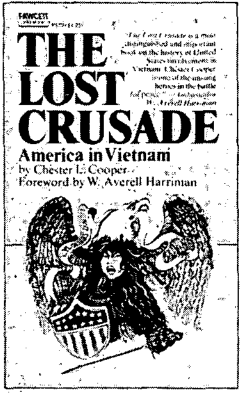

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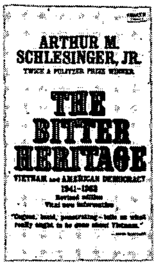

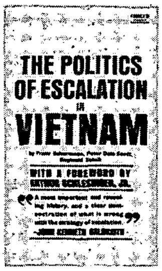

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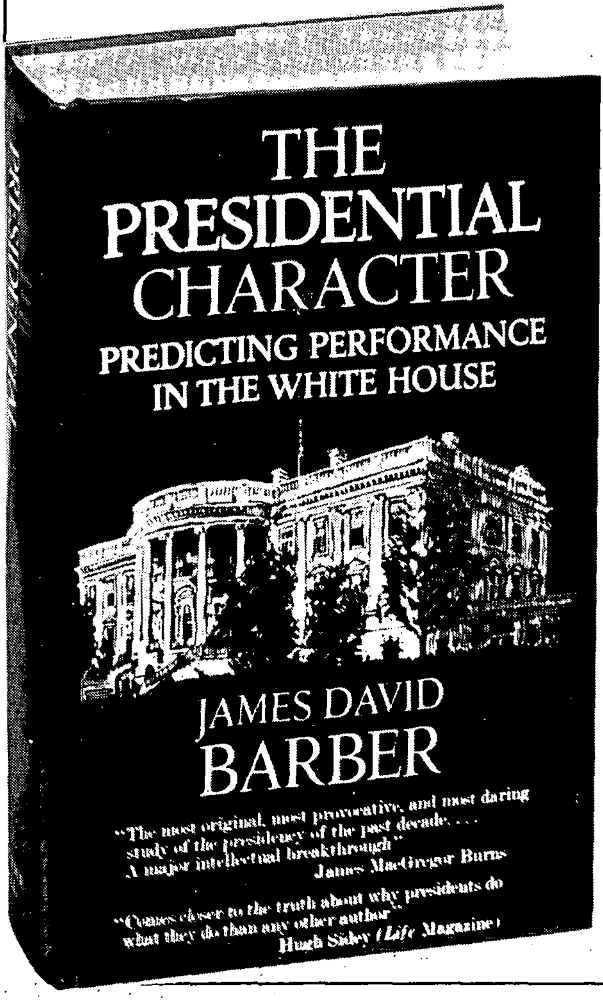
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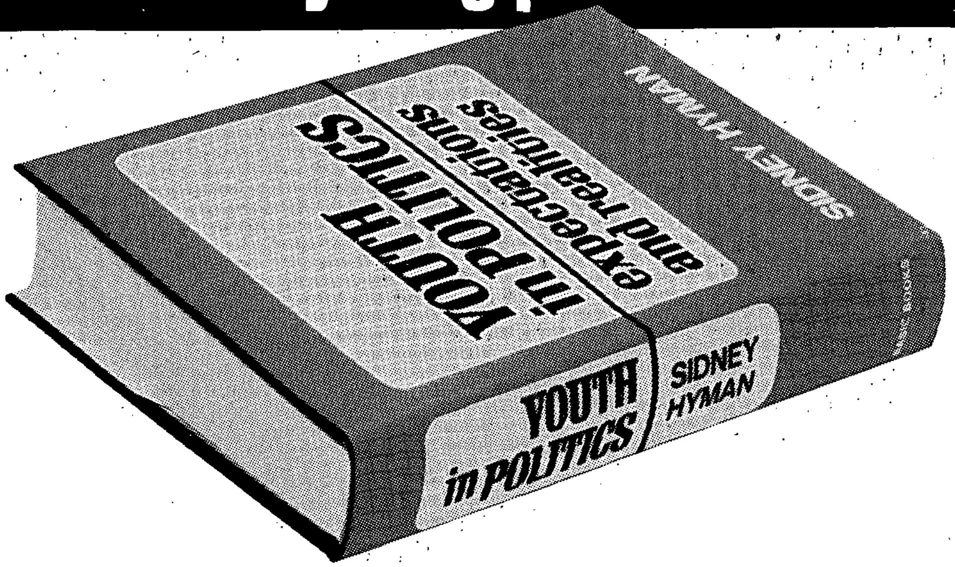

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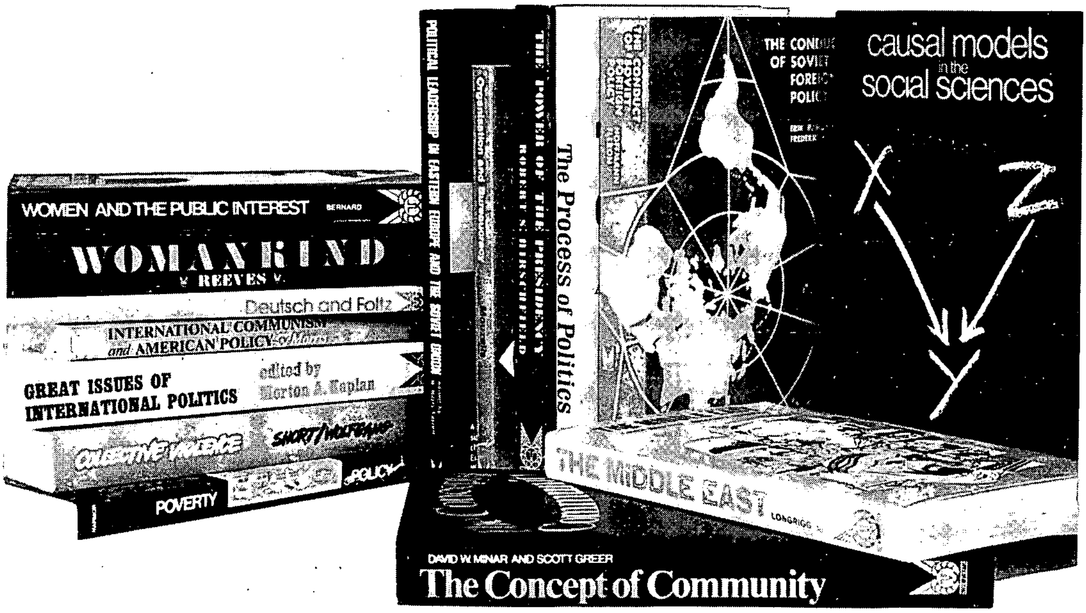
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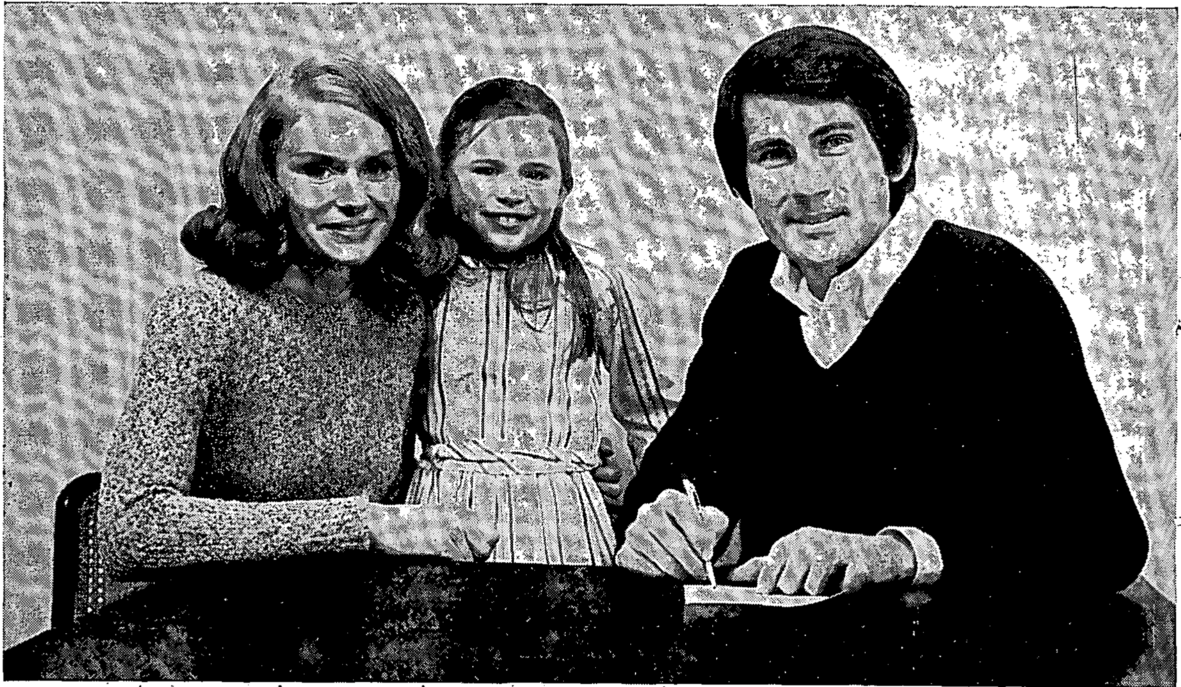

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